THURSDAY, MARCH 29, 1877

PALMÉN ON THE MIGRATION OF BIRDS

Ueber die Zugstrassen der Vögel. Von J. A. Palmén, Docent der Zoologie an der Universität Helsingfors. Mit einer lithographirten Tafel. (Leipzig: Engelmann, 1876.)

GRANTING it to be true that truth never dies, it is undeniable that error is hard to kill. A notable instance of this last assertion is furnished by the infatuation which possesses so many people, otherwise, perhaps, not unreasoning, to believe that more or fewer of the birds which commonly frequent these islands in summer, pass the winter in a torpid state—"hibernate," as they are pleased to say. Vainly have travellers or residents on the shores of the Mediterranean, or in the interior of Africa, told us over and over again, how that as the hot weather comes to an end with us, our cuckoos, our swifts, our swallows—nay, almost all our summer birds—come crowding southwards. As vainly have the same observers recorded the northward journeys of the same species, though under somewhat different conditions, on the approach of our spring. Of course, no one who merits the title of an ornithologist disregards the plain evidence thus afforded, or entertains a single doubt as to what it proves—however strongly he may recognise the fact that we know little of the paths taken by the migrants, and next to nothing of the faculty whereby they ordinarily reach their ancestral summer-home. But there are not a few persons enjoying among the vulgar of all classes the reputation of being ornithological authorities, and there are thousands of the general public, who still hanker after the ancient faith in "hibernation." It may be said that it is but lost labour to attempt to bring such people to reason, and so, possibly, it is. Still, the apparent gravity with which this absurd notion is from time to time propounded, renders it necessary that its folly should be as often exposed, lest the pertinacity with which it is urged gain for it adherents among those who think that, as they encounter no refutation of it, it may or must be true, and the testimony in its support unanswerable. As a rule, there seems to be an outbreak of the "hibernation" mania every two years or so. It nearly always presents the same essential features. Some one, who with the multitude passes for an ornithologist, sends to a newspaper a second or third-hand story of some nameless person who in some nameless place found a number of torpid swallows in the chink of a chalk-pit, or a drowsy land-rail in a haystack—or, on a log of wood being laid on the fire, of a cuckoo that woke from its slumber and, emerging from its retreat, sat on the hob, regardless of its singed plumage and cheerfully singing its accustomed song. Occasionally a brilliant imagination, and the desire of supplying some grateful novelty suggests a diversion of the details, and the swallows are dragged from a horse-pond in a casting-net, or have got themselves into an eel-pot—or the cuckoo is discovered as the billets are being split. The story, which can be fairly compared with the tales of witches' imps, and of our dear old friend the ante-diluvian toad-in-a-hole, is repeated in many newspapers, and countless correspondents write letters to their respective "organs," citing parallel cases of which they have heard from their grandmothers, and wonder why "Professor" Darwin, Mr. Buckland, or the great "Doctor" Owen, do not favour the public with their views on the matter.

A delightful example of all this occurred not many weeks since, and one, moreover, marked by so much originality of conception as to reveal the hand of a master. A reverend gentleman published the evidence of a friend's friend, or that friend's friend's friend (there was a charming uncertainty on this point, and the final friend was of course nameless), who watched "a brood of young swallows too weakly to be able to follow their parents in their migration." (Here it is to be observed that the "hibernation" advocates of late years don't deny migration *in toto*, and that, as explained by the reverend story-teller, the "swallows must have been martins!") "So the old birds left them in their nests and plastered them up with mud." To cut the story short, it is enough to observe that the ingenious and considerate parents were (as they expected) rewarded, on their return next spring, by finding their offspring "none the worse for their six months' incarceration," and after this happy ending to the tale had been told, the sympathies of the British public were duly roused, and the "hibernation" mania was ready to run its usual course. On this occasion, however, its symptoms were more pronounced than usual, and a philosophical contemporary of ours, always prone to the analysis of conduct—perhaps also seeing in the story a fresh argument against experiments on live animals—hastened to record the story among the news of the week, though admitting that it was "not much in the way of evidence." This admission, however, was prefaced by the very curious statement that "It is at least quite conceivable that a creature which had been a hibernator generations ago, and which had since discovered the preferability of migration to a warmer climate, should yet be able to return to its old habit in case of need." This remark might be allowed to pass if it had only been proved that any bird, since birds ceased to be reptiles, ever had been a "hibernator." As that is not the case it may be sent instantly to the limbo of false hypotheses. Still the admission roused the remonstrances of a correspondent of the same journal, for he not only "was inclined to think" the story "authentic," but adduced in its support an agreeable variation of the fable. His gardener had assured him "that he had himself seen what he described, "layers of young swallows in a hibernating state, when, taking up the flooring of some house in that parish [Thorpe Arch] during winter." *O fortunatos nimium!* What sights bless the eyes of gardeners! Layers of young swallows under our boards or bricks! How were the rats and mice kept from feasting on their tender bodies? And then if one did happen to die before the day of release, how sweet would be that superimposed chamber! Inviting as the theme is, we must leave it to record the further progress of this maniacal outbreak.

The next portion of our history introduces us to a new world and to a family of birds never before accused of "hibernating." A second correspondent of the same journal, writing under the honoured initials "R. N.," spins a yarn, fit for the fo'castle (if there happen to be an audience of marines), and tells how humming-birds at

Port Montt (*sic*) pass the winter in hollow trees, and are often brought into the houses cold and stiff, perfectly dormant, and yet when revived by the warmth, able to fly about the room. They only need a refrigerating ship to be brought to, and "acclimatised" in, Ireland, or kept at the Crystal Palace. This is "R. N.'s" idea, not ours, but he makes it, we doubt not, in all sincerity. We now fully expect that the next bird charged with "hibernating" will be an ostrich. The phœnix, if he could be found, would certainly not be safe, but then he is dormant already. Even now it is perhaps not too late to injure the reputation of the dodo, and announce that a Rip van Winkle of the species has been "hibernating," like a tenrec, in some secluded rift of the rocks in Mauritius.

That truth will prevail in due time there can be no doubt, and these tales of "hibernation" will serve to amuse future generations, even as that marvellous and circumstantial account of the evolution of Bernacle-geese from shell-fishes now causes mirth to us—mirth mingled with regret at the stupid credulity of our quasi-scientific forefathers. Yet hardly so. It would be an injustice to the venerable Gerard to put on a par with him these story-tellers of to-day. The old herbalist had but little light, but what little light he had he did not neglect. Our contemporaries shut their eyes and ears to that which is before them. Their wilful ignorance is absolutely criminal, therefore shall they receive greater condemnation. If any of them is open to conviction, let him reflect on this single fact. The young cuckoo, when we last see it in autumn, is clad in a plumage of reddish-brown or liver-colour. When cuckoos reappear in spring, they are, almost without exception, in their proverbial "grey." It is obvious, then, either that the young birds have moulted in the meanwhile, or else they have perished in the process of "hibernation." This latter alternative would soon put an end to the species, and cannot for a moment be entertained. But as regards the former, every physiologist will agree that while an animal is torpid, all growth is suspended—yet on the "hibernation" theory, these young cuckoos must have put off their nestling feathers, and grown those characteristic of maturity, during the time when nearly all the animal functions are at rest. Therefore it simply stands that "hibernation" in the case of the cuckoo is an impossibility. The same, too, with swallows. It is known that they renew their feathers about Christmas. The plumage of the young swallow in its first autumn does not differ so strikingly from that of the adult, as it does in the cuckoo, but any one pretending to ornithological knowledge, must know that the swallow of the preceding year can be equally declared to have changed its feathers since the last autumn, and indeed the fact of this winter-moult has been observed in caged birds, and recorded many years since by Mr. James Pearson, whose account, verified by Sir John Trevelyan, was published by Bewick eighty years ago ("Land Birds," p. 249, Ed. 1797). Hence it follows that neither swallows nor cuckoos—thus moulting in the winter months—do, as has been asserted, "hibernate."

It is indeed somewhat humiliating to be at this day refuting an error which has been so often refuted before, but necessity knows no law, and the widely-spread fallacy creates the necessity. Furthermore, this protest against the sciolism of the age has led us away from our particular object, which is to notice the remarkably careful and painstaking work of Herr Palmén, originally published in Swedish in 1874, and now appearing in a German translation, which will have many more readers. This treatise does not indeed (as will be seen from its title) profess to treat of more than one branch of the migration question. Its scope is properly limited to a consideration of the routes taken by birds of passage in their migration; but on that account it is none the less a valuable contribution to the already extensive literature of the subject, and in this German version the author appends some remarks of more general interest. He seems to have availed himself of all the information, as to his main point, that he could collect, and the wonder, perhaps, is that, living in Finland, he has been able to amass so much. His work is weak, it must be confessed, in detail as to the migratory birds of our own islands, but, as we think, from no fault of his own, since most of those who delight to consider themselves "British Ornithologists" are content to stand on the ancient ways of their forefathers, and to disregard everything that happens beyond the "silver streak" as entirely as if it belonged to another planet. Thus we doubt much if he would have greatly gained by studying the various contributions to "British" ornithology that have appeared since 1856, when the last edition of Garrell's standard work was completed. We must, however, hold that Herr Palmén's assignment of routes to the migratory birds of North-Western Europe is almost purely conjectural. We do not say it is erroneous—far from that. There is much in it which will very likely be proved true whenever British ornithological observers shall be at the pains to observe to some purpose; but, at present, his views can, from the nature of the case, be only accepted provisionally. He has far different and more solid ground to go upon when he treats of the migratory birds of Eastern Europe, and especially of the Russian Empire—whether European or Asiatic, and every ornithologist owes Herr Palmén a debt of gratitude for the compendious abstract he gives from the mighty works of Pallas's successors, and notably from those of Dr. von Middendorff.

As regards the routes taken by the migratory birds of the Palæarctic region, Herr Palmén's investigations have been so concisely summed up by a recent writer in the last edition of the "Encyclopædia Britannica" (iii. p. 768) that we take the liberty of here transcribing them as there given. These main routes are said to be *nine* in number:—

"The first (A—to use his notation), leaving the Siberian shores of the Polar Sea, Nova Zembla, and the North of Russia, passes down the west coast of Norway to the North Sea and the British Islands. The second (B), proceeding from Spitsbergen and the adjoining islands, follows much the same course, but is prolonged past France, Spain, and Portugal to the west coast of Africa. The third (C) starts from Northern Russia, and, threading the White Sea, and the great Lakes of Onega and Ladoga, skirts the Gulf of Finland and the southern part of the Baltic to Holstein and so to Holland, where it divides—one branch uniting with the second main route (B), while the other, running up the valley of the Rhine and crossing to that of the Rhone, splits up on reaching the Mediterranean, where one path passes down the western coast of Italy and Sicily, a second takes the line by Corsica and Sardinia, and a third follows the south coast of France and eastern coast of Spain—all three

paths ending in North Africa. The fourth (D), fifth (E), and sixth (F) main routes depart from the extreme north of Siberia. The fourth (D) ascending the river Ob, branches out near Tobolsk—one track, diverging to the Volga, descends that river and so passes to the Sea of Azov, the Black Sea, and thence by the Bosphorus and Ægean, to Egypt; another track makes for the Caspian by way of the Ural River and so leads to the Persian Gulf, while two more are lost sight of on the steppes. The fifth (E) mounts the Jennesei to Lake Baikal and so passes into Mongolia. The sixth (F) ascends the Lena and striking the Upper Amoor reaches the Sea of Japan, where it coalesces with the seventh (G) and eighth (O) which run from the eastern portion of Siberia and Kamchatka. Besides these the ninth (X) starting from Greenland and Iceland, passes by the Færoes to the British Islands and so joining the second (B) and third (C), runs down the French coast."

All these routes are plainly laid down on the map which accompanies the work, and in the absence of more precise information, it will hardly be in the power of any British ornithologist to dispute them, though, as before stated, we must hold them to be in a great measure conjectural. In the following chapters the author shows how necessary it is to know the principal routes taken by birds in their migrations before we can understand or reason intelligibly on their movements, and of very great interest are his remarks on the Genetic Import of Regular and Irregular Lines of Travel, and on the So-called Migratory Instinct (chaps. ix. and x.), greatly amplified in the German version from the brief paragraphs which represent them in the Swedish original. They are, however, it must be confessed, somewhat verbose; but, for all that, they are well worth reading. Though Herr Palmén refers to an article which appeared in these columns some years ago (NATURE, vol. x. p. 415), he does not seem to be aware of the theory subsequently propounded by Mr. Wallace (vol. x. p. 459) as to the possible or probable origin of migratory habits, wherein is expressed, in far fewer words than his own, what appears to be essentially the same thing. For "Migratory Instinct" Herr Palmén substitutes "Experience" as the piloting power, and though there is much to be said in favour of this explanation in many cases, others there are in which it seems to break down utterly. How do the young cuckoos which stay in this country a month or six weeks after their parents (whom, let us remember, they have never known) have departed find their way to Africa? And how do the scores, hundreds, or thousands of rapacious and wading birds, whose elders do not accompany them, manage in their autumnal journeys to arrive more or less punctually at the spot which countless generations of their predecessors have reached before them? They have had no "experience," and though doubtless many perish by the way, a very large proportion year after year hit off exactly, and at the first intention, the ancestral landing-place. What, also, can "experience," which, after all, means only a knowledge of landmarks, do for the species which travel by night, as seems to be the habit of very many birds, or for those which, like at least two of the annual visitants to New Zealand, traverse a waste of waters? At present no solution of the mystery offers itself, at present such knowledge may be too wonderful for us; but, high as it is, our faith in the progress of science forbids us to say that we cannot attain unto it.

OUR BOOK SHELF

Dynamics; or, Theoretical Mechanics, in Accordance with the Syllabus of the Science and Art Department. By J. T. Bottomley, M.A., F.R.S.E., F.C.S. (London and Glasgow: William Collins, Sons, and Co., 1877.)

THIS little text-book is issued by Messrs. Collins as one of their Elementary Science Series, and will prove useful to beginners, by rendering them familiar, at an early stage of their studies, with the more precise definitions and nomenclature which have been introduced by modern writers on dynamics. The distinction, for instance, between the centre of gravity and the centre of inertia is much more clearly pointed out than is usual in elementary works, and the statement that "there is only a limited number of classes of bodies that possess a centre of gravity" will probably be read by many with surprise. The measurement, composition, and resolution of velocities are treated of in the chapter preceding that on force, and the methods of measuring forces in terms either of gravitation units or absolute units are well and fully discussed. The definition of work given in the last chapter might, we think, be amended. As it stands at present it might lead the student to suppose that no work is done by an agent moving a body, unless the motion is created in opposition to a resisting force, though the language employed in some of the examples would be sufficient to correct such a supposition. Throughout the work the author assists the student to obtain "clear physical conceptions regarding the first principles of dynamics," by frequently directing his attention to the experimental proofs of the various laws he enunciates, and by hinting at the physical, rather than the mathematical, developments of his subject.

On these grounds, we have formed a very favourable opinion of Mr. Bottomley's work, and we have no doubt that it will meet with the success it deserves among a wider class of students than that for which it is specially designed. A. R.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

Evolution and the Vegetable Kingdom

MR. CARRUTHERS has embodied in the *Contemporary Review* the substance of his Presidential addresses to the Geologists' Association, on which we would offer a few points for consideration.

Although not agreeing with Mr. Carruthers as to the inferences to be drawn from the present state of our knowledge of fossil vegetable remains, we cannot but admire the earnestness with which he makes a stand in what we regard as a losing cause. We set a high value on his researches in fossil botany, and his work is characterised by unvarying and careful exactitude. Whatever may be his theories, his reputation will rest on a solid basis of work. Palæontologists have to thank him for unvarying kindness and readiness to aid them in their researches, forming a marked exception to the treatment which botanists usually give the subject.

In discussing this question, we must keep well in mind the teaching of Sir Charles Lyell, first as to the insufficiency of the geological record, especially with regard to land-surfaces. Considering the denudation and the wasting action of the waves to which remnants of terrestrial conditions are exposed during the slow process of their submergence beneath the sea, and again during their gradual upheaval, it is surprising to us not that so few records are preserved, but that any vestiges whatever remain. Secondly, with regard to lapse of time, we must get the "chill of poverty out of our bones," and not misinterpret "the sign of successive events, and conclude that thousands of years were implied where the language of nature imports millions." Mr. Carruthers admits the imperfection of the geological record, although scarcely with sufficient emphasis, and compares its fragmentary condition to a tablet containing

the remains of an unknown inscription represented by only a few of its numerous letters, each of which occupies its proper relative position to the known and unknown letters of the inscription. This is hardly a happy simile as the relative ages of the beds and strata containing vegetable remains, scattered over the world, are certainly very far from settled, and their correlative sequence is in numerous instances still the subject of great discussion. The relative position of these letters is therefore at present but vaguely known.

Haeckel supposes "that the sub-marine forests of the primordial period were formed by the huge brown algæ or fucoidæ." In the 70,000 feet of sedimentary rocks, from the Laurentian to the Devonian, beds of carbon and graphite are abundant; the only known vegetation throughout that period is of algæ. So far only Mr. Carruthers agrees with Haeckel; from this point his views diverge. During the period of the deposition of these 70,000 feet, time enough surely elapsed (if time only be required) for the evolution of vascular cryptogams from algæ. In the Silurian had vegetation been equally fitted to resist decay, we should probably have had plant evolution indicated side by side with that of animals. Before leaving the subject of algæ, we must differ from Mr. Carruthers, who says that if rich floras had existed, the limestones of the Llandovery rocks at Malvern would have preserved them. In these marine rocks at the most sea-weeds could be expected, and limestone of whatever age do not usually preserve such traces; but we know that floras existed by the carbon and graphite before-mentioned.

Mr. Carruthers urges what he considers as fatal objections to the doctrine of evolution; his arguments may be briefly stated as follows:—

1. The simultaneous appearance of the three principal groups of vascular cryptogams, even in a more highly organised condition than their living representatives.

2. The early appearance of gymnosperms and the want of connecting links between these and the lycopods from which they are supposed to have been developed.

3. The early appearance of monocotyledons.

4. The sudden appearance of dicotyledons, not only in the lower form as Apetalæ, but also as Dialypetalæ and Gamopetalæ.

5. The persistence in specific character of *Salix polaris* from the glacial period until now and over a wide range.

Let us now see on what facts these objections are founded, and whether the facts are not open to other interpretations.

1. The evolution theory requires that lower groups have developed until the amount of organisation was reached required to enable them to fulfil the conditions under which they live, and to occupy vacant ground in the economy of nature. This required amount of development may be more or less quickly attained, and the development of the organism then remains almost stationary. Side by side with this development, other development goes on unceasingly, leading to the gradual evolution of entirely different and more highly organised forms. The cryptogams are paralleled amongst vertebrates by the early and specialised development of reptilia; amongst crustacea by trilobites, &c. In like way the tetrabranchiate cephalopods, the brachiopods, and numerous other mollusca, whose hard shells resisting decay, have enabled us to trace their life history, have come down to the present time, just as these vascular cryptogams have, not in their most complex and differentiated forms. We do not expect to find sustained progressive development in the lower animal and vegetable groups, and are not surprised at evidence of actual reversion.

2. On their first appearance "the Gymnosperms do not," Mr. Carruthers says, "present a generalised type, but a remarkable variety of genera and species, all as highly differentiated as any of the existing forms." Now if this is absolutely the case, and their *first appearance* in life is coincident with their first appearance in the fossil record, there is no doubt that they were specially created, and there is no need of further argument. But the point not yet proved is that the two are coincident. The occurrence of coniferæ in the Devonian is only known by wood with coniferous structure. The fruit and foliage, if known, might possibly afford an indication of the mode in which their course of evolution, as suggested by Haeckel, had taken place. Unger has described anomalous woods from Thuringian rocks of Devonian age. "Had these been of earlier age than Miller's Cromarty wood they might have been looked upon as one of the steps leading up to the true Coniferous structure." These may yet be looked on as steps, for the relative age of these Devonian rocks is still to be fixed. This occurrence of anomalous wood is at all events not to be overlooked, if, as is stated, the gymnosperms both structurally and embryologically form the transition group from ferns to angiosperms. The occurrence of Coniferous wood in Devonian rocks rather shows how great are the gaps to be filled up, and that the evolution of the gymnosperms commenced at an earlier period than was supposed, during the formation of the great carbon layers of the older rocks, and side by side with the development of ferns and lycopods. The common ancestors of the spore-producing lycopod and the seed-bearing gymnosperm are to be sought in remoter times even than the Devonian. There is no evidence that the Devonian woods were those of the higher and diœcious conifers and that conifers first appeared in this form. Little is really known of the earlier coniferæ, but the cycads—the lowest form, and most nearly allied to ferns—were far more abundant formerly than at present. Schimper writes as follows:—"What form the prototype of our conifers took in carboniferous times is not satisfactorily settled, neither fruit nor foliage having been discovered which could be placed in any order with certainty. The few fragments placed in Abietinæ may belong to Lepidodendron."[1] In the Permian rocks conifers are abundant.

3. Concerning the appearance of Monocotyledons at the base of the Trias, the first true monocotyledon, Mr. Carruthers states, is the stem and spike of an aroïdeous plant from the lowest carboniferous strata near Edinburgh.[2] Recently a number of additional specimens have come to light, but Mr. Etheridge, junior, who is referred to by Mr. Carruthers as having found them, does not we believe regard them to be monocotyledons at all; and in this view, although we have not examined them, we are inclined to concur, because it seems unlikely that so many spikes should be found without foliage. In the *Transactions* of the Botanical Society of Edinburgh, vol. xii. p. 152, Mr. Etheridge points out that the stem of Pothocites was branched, and what was thought by Paterson to be the remains of a deciduous spathe was one of a series of small enlargements which "occur along the course of the stem at regular intervals, jutting out one on each side opposite one another." This additional information throws still greater doubt on the correctness of the determination. At Bournemouth, where aroids are abundant, leaves only have been found without a single spike. This is not the first time monocotyledons have been supposed to be present in the Carboniferous; for example, Cordaites, a plant now acknowledged to be a gymnosperm—but whether a cycad or conifer is still, according to Schimper, a matter of doubt—was formerly supposed to be a palm, and subsequently a Yucca or Dracæna. The curious twisted bodies called Spirangium are assumed to be monocotyledons on very slender grounds, their affinities, according to high authorities, being completely unknown. Nevertheless we find Mr. Carruthers, referring to the Carboniferous, says:—"Including these fruits there are probably eight species of monocotyledons in the later Palæozoic rocks." But *excluding* them there is but one, and that, as just shown, of an extremely doubtful nature. Monocotyledons occur doubtfully in the Trias as Yuccites, and in many forms in the Lias, agreeing so far with Haeckel's table of their pedigree. They gradually increase in number until the present day. Although we question the reality, we think the early appearance of a monocotyledon, even if it had occurred, would no more invalidate the theory of evolution than does the equally unlooked-for occurrence of mammalian remains in secondary rocks, invalidate the theory in reference to animal remains.

4. The next point Mr. Carruthers brings before us is the appearance of dicotyledons, and as their testimony for or against evolution is very important, this testimony deserves examination at some length. Mr. Carruthers regards, as the most fatal objection to the evolution theory, the supposed fact that representatives of all the three great groups appear simultaneously in the Upper Cretaceous rocks. Dicotyledons have been found as low down as the Neocomian, and their discovery in rocks of this age is quite recent. Still the evidence that this is their earliest appearance is purely negative, and no hypothesis is satisfactory which is based entirely on negative evidence. It is probable that dicotyledons may be found in yet earlier rocks—perhaps quite early, although playing an extremely subordinate part. The Wealden has yielded no monocotyledons, yet we know that they must have existed; may not then the earlier forms of dicotyledons also have existed? We may parallel the case of the mammals from the Purbecks. The Purbeck fauna was considered to show no trace of mammals until the examination of a particular

[1] *Pinus anthracina*, Lindley and Hutton, is "certainly a fragment of a Lepidodendroid fruit."—Carr., *Geol. Mag.*, vol. ix. p. 58.

[2] *Pothocites grantoni*, Paterson *Trans. Bot. Soc. Edinb.*, vol. i. p. 45, pl. 3, f. 1-3. Not mentioned by Haeckel or Schimper.

small area of the Purbeck beds revealed their presence in numbers; had this spot not been quarried it would have been supposed for years that mammalia had made their appearance in Eocene times. Some causes, tending to make the preservation of dicotyledons difficult were discussed in NATURE, vol. xv. p. 281, and need not be further alluded to here. Mere localised patches of plant remains are not an unerring index of the character of a flora at any period. At Bournemouth there are patches just underlying the lowest marine beds, which are crowded with ferns only; other patches contain nothing but ferns, aroids, and gymnosperms. Had these patches been isolated, inferences of a most misleading character would have been drawn.

The upper Cretaceous floras are known to us principally from Aix-la-Chapelle and from America; but as in both these—indeed in most cases—the supposed Cretaceous beds containing plant remains rest on palæozoic rocks, their relative age is a matter of uncertainty. M. Barrois fixes it as contemporaneous with his zone of Belemnitella, but whether he is right in this supposition or no', the flora contains ferns and other plants which seem identical with those of the Bournemouth beds. In America, in the Dakota group, we have leaf beds 400 feet thick of the supposed age of our gray chalk, but the associated marine beds have, mixed with decidedly Cretaceous forms, shells approaching very closely those of our London clay. It seems more logical to determine the age of a rock by the incoming of new types than by the lingering of old, and the whole palæontological evidence shows that these beds are at most intermediate in age between our Eocene and Chalk, the enormous gap between which is probably filled up here by some 2,200 feet of strata. American geologists are not agreed as to their age. It would be out of place to discuss this subject at length, but enough is said to show that the relative ages of these floras is not definitely known, and that no series of arguments based on their relative sequence is, at present, entitled to any weight. M. Lesquereux finds evidence in support of evolution in the flora of Dakota, "in the remarkable disproportion of genera compared to species;" and in the sameness of the leaves, which are "mostly entire, coarsely veined, and coriaceous, the difficulty of separating them into distinct groups, by fixed characters, the numerous forms of leaf which, seen separately, represent different species, or even genera, and which, considered in series or groups, appear undividable into sections." When, however, he theorises, we see that he makes use of the same arguments against evolution as those put forward by Mr. Carruthers. Von Ettingshausen, on the other hand, who has paid much attention to the subject, states that he is able to trace the ancestry of our present floras back to simple elements in Tertiary times, and these to still simpler and more united types in Cretaceous times. In his works a number of examples are given. The flora of Sezanne, whose age as Lower Eocene may be accepted, is closely analagous with that of Bournemouth.

Now let us examine the manner in which determinations of fossil leaves from these earlier rocks have been made, and see whether they are sufficiently reliable to entitle us to form any theories whatever as to the simultaneous appearance of the three divisions of Dicotyledons. Let us take the flora of Dakota.

Of *Polypetalæ* we have Liriodendron, founded on two fragments, and Magnolia on two fragments. These fragments are of simple leaves and possess no character whatever in themselves, upon which they can be determined. Magnolia, for instance, is determined from the similarity in form to leaves described as Magnolia by Heer in the Flora of Greenland, which themselves are supposed to be Magnolia because they resemble (not specifically) Magnolias from the Miocene of Europe. In Menespermites, the third genus, the name indicates that its affinities are vague, and we accordingly see that it had been formerly described as Dombeyopsis, Acer, Populites. The *Gamopetalæ* are represented by three genera. Of these Andromeda is determined on two fragments and one indistinct leaf of simple lanceolate form; Diospyros, formerly described as Quercus, is determined from one simple and ovate leaf resembling Laurus, the other a round and simple leaf; while Brumelia is still more unsatisfactory, and has been previously thought to be either Laurus or Quercus. The determinations have been changed, as we see by the position of the plates and the figures on the plates, many times during the progress of the work, and it is not too much to say that all the determinations of leaves of Polypetalæ and Gamopetalæ from this flora are vague and unsatisfactory, and no one would be more ready to acknowledge this than Mr. Carruthers himself. We do not find fault so much with the determinations themselves, which are probably the best that could be made from such material, but we think it premature to base any theories upon them as to the simultaneous appearance with the Apetalæ of the more highly organised Dicotyledons.

In the Eocene and Miocene we have, however, richer materials, and the variety and completeness of the fossil flora become conspicuous; the forms, as Lyell says, "were perfect, changing, but always becoming more and more like, generically and specifically, to those now living." Von Ettingshausen has traced the direct descent of many living species back to the Miocene, sometimes two or more species to a common parent stock.

5. With regard to the persistence of *Salix polaris*, it appears to be simply a case of a plant becoming thoroughly adapted to certain conditions of life which were met with in England during the glacial period, and are present now in extreme northern regions. Why *Salix polaris* should have varied since glacial times more than mollusca and other animal life is not apparent. The intermediate forms which should connect willows and poplars have not been found, but as poplar-like leaves have been met with in lower cretaceous rocks, it is probable that the order of Salicaceæ is an extremely ancient one, and the single generalised form must be sought for in remoter times even than the Cretaceous.

Our general broad knowledge of the succession of plant life, as testified by the rocks, is too well known to need recapitulating here. Schimper enters in detail into its history. In the Silurian, Algæ; in the Devonian ferns and Lycopods, reaching their apogee of development in the Carboniferous; and in the Permian the conifers first take an important position. The Triassic indicates a great gap, and may be considered the reign of gymnosperms, whilst the incoming of the phanerogams is placed beyond doubt. The Jurassic presents another hiatus, and but little is known of its flora.[1] Heer, however, infers, from the entomological fauna, that there were no leafy trees in the Lias. The oolitic rocks contain abundance of cycads. The Wealden and Neocomian vegetation has left us little more than gymnosperms and ferns. With the upper cretaceous period dicotyledons are abundant, but their incoming is traced to older rocks. The Eocene contains rich assemblages of dicotyledons, principally apetalous, and the Miocene, better known, a still greater variety. We see the same plan of development in the individual; and, as Prof. Huxley recently stated in a lecture at South Kensington, "we can trace living plants from the most gigantic and complicated tree, step by step down through many gradations to the lowest algæ, the lichens, and on down to a piece of animal jelly."

Thus we find on reviewing the evidence that has been brought forward, that other interpretations may be put upon the facts presented to us by Mr. Carruthers. J. S. G.

IN an article in this month's *Contemporary*, entitled "Evolution and the Vegetable Kingdom," Mr. Carruthers refers incidentally to a question that deserves the careful consideration of all who accept the doctrine of evolution; viz., whether the earliest type of flower was hermaphrodite or unisexual. Alluding to the abundance and variety of palæozoic gymnosperms, as evidenced by the numerous fruits that have been discovered in the carboniferous measures, he lays stress on the fact that "they all belong to the Taxineous group of conifers . . . . that the plants of this section are all dicœious, *i.e.* having the sexes on different plants. If the occurrence of the germ and sperm elements in different organs, and even in different individuals, is evidence, as it is held, of higher development in phanerogams, then it is important to notice the order of appearance of dicœcious and moncœcious groups in relation to those with hermaphrodite flowers. Advocates of evolution hold that dimorphic plants are now in a transition stage progressing towards a dicœcious condition. The conifers attained to the highest known development as regards this element of their structure on their first appearance."

If Mr. Darwin be regarded as an exponent of the views held by "advocates of evolution," we find that he expresses himself very differently. From the following passages in his recently published work on "The Effects of Cross and Self-fertilisation in the Vegetable Kingdom," he would seem to consider the primordial condition to be unisexual. "There is good reason to believe that the first plants which appeared on this earth were cryptogamic. . . . . As soon as plants became phanerogamic and grew on the dry ground, if they were to intercross, it would be indispensable that the male fertilising element should be transported by some means through the air; and the wind is the

[1] An extensive Jurassic flora has been described by Heer in *Mém. de l'Acad. Imp. des Sciences de St. Pétersbourg*, vii^e^ série, tome xxii. No. 12, 1876.

simplest means of transport." . . . . "Therefore the Coniferæ and Cicadiæ, no doubt, were anemophilous, like the existing species of these groups." . . . . "A remarkable fact with respect to anemophilous plants is that they are often diclinous." For reasons which he gives, Mr. Darwin considers that this "may be attributed to anemophilous plants having retained, in a greater degree than the entomophilous, a primordial condition, in which the sexes were separated and their mutual fertilisation effected by means of the wind." . . . . "If this view is correct, plants must have been rendered hermaphrodites at a later though still very early period, and entomophilous at a yet later period, namely, after the development of winged insects." He subsequently points out, however, that "under changing conditions of life . . . . some hermaphrodite plants, descended, as we must believe, from aboriginally diclinous plants, have had their sexes again separated;" and he names as an example, *Lychnis dioica*. It is only in the case of plants thus reverting that dimorphism can be held to be a transitional stage.

Prof. Thiselton Dyer, in his notice of Mr. Darwin's book in NATURE (vol. xv., p. 329), maintains an opposite view. "It would not be difficult to show that all through the vegetable kingdom the hermaphrodite condition precedes the dicœcious." Demurring to Mr. Darwin's conclusion that the monœcious condition "is probably the first step towards hermaphroditism," he considers it "not improbable that precisely the converse may be more true." . . . . "To throw light on the question whether the primordial plant was diclinous or not," he discusses the manner in which it probably originated "from some plant-form not distantly related to Selaginella," and arrives at the conclusion that the first flower would probably be extremely inconspicuous, destitute of colour and hermaphrodite.

How would it be, however, if, instead of regarding the sporangiiferous cone or spike of Selaginella as the homologue of a single flower, we compare it rather with the spike of Carex, say, for example, *C. pulicaris?* The spike in this species is, like the other, "composed essentially of an axis having modified lateral appendages." The glumes of the sedge correspond to the scales of the lycopod; in the axils of the upper are found the "male structures"—in Selaginella, sporangia containing microspores; in Carex, anthers containing pollen; in the axils of the lower are found the "female structures"—in Selaginella, sporangia containing macrospores; in Carex, ovaries containing each an ovule. There is then not even the difference that the position on the axis of the male and female structures is inverted. From *C. pulicaris*, with its single spike, the passage is easy to species that have several spikelets, each male at the top and female below; or to others that have the upper spikelets wholly male, the lower ones wholly female. The same arrangement of male and female elements is found in Typha and Sparganium, in most of the Araceæ, to which order belong the oldest fossil monocotyledons, and is preserved even in Sagittaria, although in the last the flowers are of a much higher type, being provided with petalloid perianths. So far as the comparison with Selaginella is concerned, does it not favour the production in the first place of unisexual flowers, at least as much as of hermaphrodite?

THOMAS COMBER

Newton le Willows

The Rocks of Charnwood Forest

SOME letters appeared in NATURE a few months ago upon the rocks of Charnwood Forest. In one of them it was suggested that the syenitic bosses of Markfield and Groby might be more ancient than the surrounding slates and grits. Some of your readers may therefore be interested in learning that we have now ascertained from unquestionable evidence in two places that the syenite is intrusive in these rocks, and, as we believe, in some of the highest and latest rocks of the series. We reserve the details of the sections and localities for a paper which we hope shortly to communicate to the Geological Society.

St. John's College, Cambridge, March 20

T. G. BONNEY
E. HILL

Southern Double Stars

NOTING some queries in your November numbers (in the "Astronomical Column") respecting some southern stars, I inclose you some extracts from our occasional observations that refer to the objects named in those and some previous numbers:—

Southern Double Stars.—Measures with 8-inch Refractor.

	Pos.	Dist.	No. Obs.	Epoch.	
	°	″			
p Eridani	237·3	5	2	1877·03	
α Centauri	50·6	3·9	3	1876·72	
γ Centauri	8·5(?+180)	1·3	1	1876·63	
B A C 1972 = h 3835	10·0	2·1	5	1876·98	
γ Argus 2nd * ...	214·8	42·5	2	1877·03	Magnitudes 2–4½–8–9½
„ 3rd * ...	151·1	62·6	2		
„ 4th * ...	141·3	94·3	2		

ROBT. L. J. ELLERY

Melbourne Observatory, January 22

Ship's Chronometers

WE have read with much pleasure your notice (vol. xv. p. 403) of Sir William Thomson's lecture on Navigation, and are prepared fully to endorse your remarks as to the value of Mr. Hartnup's system of rating ships' chronometers, by which account is taken of the change of rate due to change of temperature.

It is but fair to mention, however, that the principle upon which this system is founded was thoroughly investigated by experiments upon a large number of chronometers by M. Lieusous, of Paris, some thirty or forty years ago; acting upon his suggestions, and after independent investigation conducted in our chronometer manufactory some six years ago, we produced a table for the use of captains and others using ships' chronometers, which was fastened in the chronometer case, with a small thermometer in front of it, in such a way that the top or upper end of the column of mercury indicated, without any calculation whatever, the mean rate that should be given every day to the chronometer until some considerable change of temperature had taken place (say 3°), when the new position of the top of the mercury column again showed the new rate to be used in working the chronometer.

We did not introduce this plan to the navigating public generally, as we feared that sufficient trouble (small though it actually be) would not have been taken in the use of it, also for another reason, *i.e.*, nearly every chronometer that we have tested has been found to require a different daily coefficient for a change of temperature of ± 1°, and ships rarely remain sufficient time in port for us to determine this coefficient after allowing for the time necessary to clean the chronometer.

But, as our system of tabulation may be of interest to your readers and may possibly be available for other purposes, we give it you as briefly as possible.

From our experience (which agrees with that of Lieusous and of Hartnup) we find that the ordinary compensation balance without auxiliary, causes the chronometer to go at its fastest rate (or in other words to *lose least*) at a point of the thermometric scale somewhere between 55° and 70° F., usually at 60° or 65°, and that from 25° to 30° above or below that "fastest point" the chronometer loses or goes slower on its fastest rate by an amount that is determined by multiplying the square of the difference in temperature between the new point and the "fastest point" by the coefficient of temperature for a change of 1° above or below the "fastest point," *i.e.*, by the amount that a chronometer goes *slower* for having its temperature increased or diminished by 1°.

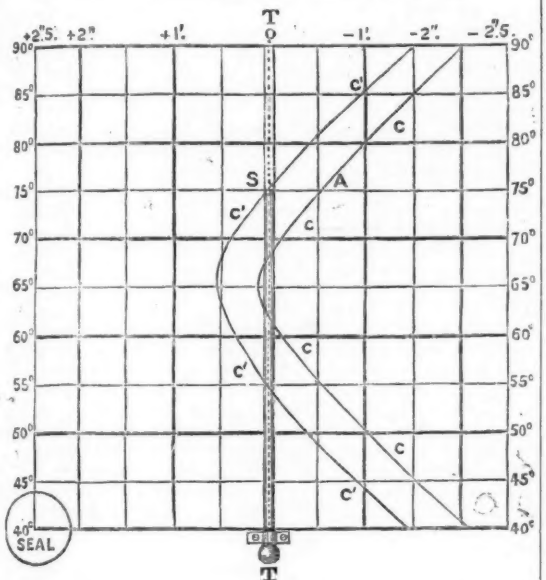
This is, however, only true so long as two conditions are adhered to; the first condition being that the "fastest point" (which can be moved at will by the chronometer maker), shall not be taken outside the limits of 55° to 70°, that is, the rule will apply from 30° to 95° but not beyond, even though the "fastest point" should be moved up to 80° or down to 40°; beyond these points a new law applies, and instead of squaring the difference of temperature and multiplying that quantity by the coefficient of temperature, this latter must be multiplied by the difference of temperature (always reckoned from the "fastest point" of the chronometer under trial) raised to a power between the square and the cube, or $x^{\frac{5}{2}}$, for which good reasons can be given.

This being explained, it will be easy to perceive the use of the

table or diagram as given below, in which T is a thermometer, C C and C′ C′ are curves drawn after testing the chronometer. For an ordinary voyage in which no extremes of temperature are expected, or which, if they occur, will be of short duration only, we should seal the diagram under the thermometer, so that the temperature line of 65° should coincide with and pass through the apex of the curve, and if the chronometer were neither gaining nor losing at 65° we should draw the curve as C C; if the chronometer were gaining five-tenths of a second per day we should draw the curve as C′ C′.

Then the rate is always to be reckoned from the summit of the mercury horizontally till the line meets the curve; if this line should be to the left of the thermometer the time should be reckoned as *plus* (+) or *gaining;* if to the right, as *minus* (−) or *losing*.

Thus, for example, taking the line S A for a chronometer whose rate at 65° is 0″·0ths, this will give at 75° − 0″·5ths, or losing half a second daily; or for a chronometer whose rate at 65° (C′ C′) is fast 0″·5ths, at 75° it would be 0″·0ths for a chronometer whose daily coefficient gives a curve as here drawn.



Of course in determining a daily rate, two or more observations of temperature should be taken, so as to give a mean temperature point from which to reckon the rate, as the day and night temperatures differ considerably.

Prof. Lieusous, in his *brochure*, gives a rule for determining the amount which a new chronometer is likely to *gain* on its rate, owing to the hardness of the balance-spring and other causes independent of temperature, but we do not find this latter so reliable as the temperature-correction method as detailed above.

Should this prove interesting to your readers, we may, with your permission, at some future time give a few reasons for the difference that is found to exist between the daily coefficients of temperature of different chronometers.

PARKINSON AND FRODSHAM

4, Change Alley, Cornhill, London, March 12

P.S.—The above system renders the auxiliary compensation unnecessary, and can therefore effect a saving of 4*l.* to 5*l.* on the cost of each instrument.

Lowest Temperature

THERE appears to be something almost abnormal in the climatic conditions to which the observatory at Stonyhurst is subject (vol. xv. p. 399). I remember going into a garden in the neighbourhood of Knaresborough, in Yorkshire, about eight o'clock on the morning of Christmas Day, 1860, and seeing what I suppose had never been seen in England outside a laboratory before that morning, viz., the mercury in a thermometer standing at 8° F. below zero, *i.e.*, 40° F. of frost. At Stonyhurst on the same day the thermometer went down only to 6°·7 F., *i.e.*, there were 25°·3 F. of frost.

Again, on March 1, 1877, the lowest temperature registered in the neighbourhood of Knaresborough was only, I believe, 18° F., whilst at Stonyhurst it was 9°·1 F. The differences, therefore, between the temperatures on the two days spoken of at these places, not fifty miles distant from each other, were respectively 2°·4 F. and 26° F., which are so wide apart as to suggest that Stonyhurst is subject to climatic conditions which do not prevail in the Vale of York. I may mention that the record in the *Times* of the temperature on the morning of March 1, was only 25° F., but in country districts in the south of England it was as low as 20° F. Great numbers of oaks, laurels, and other evergreens were killed in Yorkshire by the frost of 1860.

Oxford R. ABBAY

Meteor

A FEW minutes before 10 o'clock on Saturday night I saw a very beautiful meteor towards the western horizon. The meteor passed obliquely downwards towards Orion's belt, moving slowly from right to left. When first seen it was a brilliant white body about ⅛th the apparent diameter of the moon. As it passed onwards it became bluish and pear-shaped with a bright track. Before its final disappearance between the belt and the pleiades it had a purplish hue. It was visible about four or five seconds, and during that period it traversed about ten or fifteen degrees.

Brighton, March 12 W. AINSLIE HOLLIS

I SAW the meteor at 9h. 56m. P.M. of Saturday, March 17, mentioned by your correspondent, "W. M." I was on the sea-shore at Rossall, near Fleetwood, and while looking out to sea, due west, I became aware of a sudden outburst of light on my left. On turning I saw the splendid meteor sailing rather slowly across the sky from a point about 3° north-west of ε Hydræ to a point in Monoceros, whose position I should estimate to be about R.A. = 7h. 30m.; Decl. = 20° 0′ south.

March 26 J. H.

DR. SCHLIEMANN ON MYCENÆ

LAST Thursday night will be always regarded as a memorable one in the history of the Society of Antiquaries, when Dr. Schliemann described to an unusually distinguished audience his own and his wife's explorations on the site of the Acropolis of ancient Mycenæ. Taking as his clue the well-known passage in which Pausanias (A.D. 176) speaks of the ruins and traditions of the famous Greek city, Dr. Schliemann was led to the belief that his scholarly predecessors had mistaken its drift. The passage in Pausanias runs thus:—

"Among other remains of the wall is the gate, on which stand lions. They (the wall and the gate) are said to be the work of the Cyclopes, who built the wall for Pætus in Tiryns. In the ruins of Mycenæ is the fountain called Perseia, and the subterranean buildings of Atreus and his children, in which they stored their treasures. There is a sepulchre of Atreus, with the tombs of Agamemnon's companions, who on their return from Ilium were killed at dinner by Ægisthus. The identity of the sepulchre of Cassandra is called in question by the Lacedæmonians of Amyklæ. There is the tomb of Agamemnon and that of his charioteer Eurymedon. Teledamos and Pelops were deposited in the same sepulchre, for it is said that Cassandra bore these twins, and that, when still little babies, they were slaughtered by Ægisthus, together with their parent. Hellanikos (B.C. 495–411) writes that Pylades, who was married to Electra by the consent of Orestes, had by her two sons, Medon and Strophios. Clytemnestra and Ægisthus were buried at a little distance from the wall, because they were thought unworthy to have their tombs inside of it, where Agamemnon reposed, and those who were slain with him."

Previous explorers had searched in vain for any of the relics here referred to, because they searched in the wrong place, mistaking the wall spoken of for that of the city,

whereas Dr. Schliemann's instinct led him to infer that Agamemnon and his companions were buried within the wall of the citadel. Following this clue he began three years ago to sink many shafts in different parts of the Acropolis, and met with such encouraging results near the Lions' Gate mentioned by Pausanias that he devoted his main attention to diggings in this quarter. There were, however, so many hindrances, that it was only in last July he was able to carry out his plans.

In the Acropolis Dr. Schliemann had entirely cleared the famous Lions' Gate, which he went on to describe, discussing also the old question of the symbolism of the lions surmounting the gateway, and of the altar surmounted by a column, on either side of which rest the fore paws of one of the two lions. One theory was that the column related to the solar worship of the Persians, another that the altar is a fire altar, guarded by the lions; a third that we have here a representation of Apollo Agyieus. Dr. Schliemann himself was of this last opinion, which, he thought, was borne out by the Phrygian descent of the Pelopidæ. The lion-cult of the Phrygians was well known. Besides, among the jewels found in the tombs, and especially in the first tomb, this religious lion symbolism re-appeared. On two of the *repoussé* gold plates there found was seen a lion sacrificing a stag to Hera Βοῶπις, who was represented by a large cow's head, with open jaws, just in the act of devouring the sacrifice. On entering the Lions' Gate were seemingly the ancient dwellings of the doorkeepers, of whom some account was given. Further on, as at Troy, was quadrangular Cyclopean masonry, marking the site of a second gate of wood. Still further on were two small Cyclopean water-conduits; to the right of the entrance passage were two Cyclopean cisterns. A little further on came to light that large double parallel circle of closely-jointed, slanting slabs, which has become so famous during the last three months. Only about one-half of it rests on the rock, the other half rests on a 12-feet high Cyclopean wall, which has been expressly built to support it in the lower part of the Acropolis. The double circle had been originally covered with cross slabs, of which six are still *in situ*. Inside the double slabs was, first, a layer of stones for the purpose of holding the slabs in their position. The remaining space was filled up with pure earth mixed with long thin cockles, in the places where the original covering remains in its position, or with *débris* of houses mixed with countless fragments of archaic pottery wherever the covering was missing. This circumstance could leave no doubt that the cross slabs were removed long before the capture of Mycenæ by the Argives (B.C. 468). The entrance to the double circle was from the north side. In the western half of the circle Dr. Schliemann discovered three rows of tomb stelès, nine in all, made of calcareous stone. All stood upright; four only which faced the west had sculptures in relief. One stelè, precisely that beneath which was found the body with the golden plates representing the lion sacrificing the stag to Hera Βοῶπις, represents a hunting scene. The two next sculptured sepulchral slabs represent each a battle scene. The Mycenæ slabs, Dr. Schliemann said, were unique of their kind. The manner in which they fill up the spaces not covered by men and animals with a variety of beautiful spiral ornaments reminds us of the principles of the painting on the so-called Orientalising vases. But in the Mycenæan sculptures nowhere do we see a representation of plants so characteristic of ancient Greek ornamentation of this class. The whole is rather linear ornamentation, representing the forms of the bas-relief. Hereby we have an interesting reference to the epoch in Greek art preceding the time when that art was determined by Oriental influences, an epoch which may approximately be said to reach far back into the Second Millennium (B.C.).

Here then in the Acropolis of Mycenæ are tombs which are no myth, but an evident reality. Who were these great personages entombed here, and what were the services rendered by them to Mycenæ which deserved such splendid funereal honours? It was argued at length that the inhabitants of these tombs could be none other than the very persons spoken of in the extract Dr. Schliemann had cited at the outset from Pausanias. Dr. Schliemann then proceeded to state the details of what he had found below the ruins of the Hellenic city. He spoke of the vast masses of splendidly archaic vases. Iron, he remarked, was found in the upper Hellenic city only, and no trace of it in the prehistoric strata. Glass was found now and then in the shape of white beads. Opal glass also occurred as beads or small ornaments. Sometimes wood was found in a perfect state of preservation, as in the board of a box (νάρθηξ), on which were carved in bas-relief beautiful spirals. Rock-crystal was frequent, for beads and also for vases. There were also beads of amethyst, onyx, agate, serpentine, and the like precious stones, with splendid intaglio ornamentation representing men or animals. When towards the middle of November he wished to close the excavations, Dr. Schliemann excavated the spots marked by the sepulchral slabs, and found below all of them immense rock-cut tombs, as well as other seemingly much older tombstones, and another very large sepulchre from which the tombstones had disappeared. These tombs and the treasures they contained, consisting of masses of jewels, golden diadems, crowns with foliage, large stars of leaves, girdles, shoulder-belts, breast-plates, &c., were described in detail. He argued that as 100 goldsmiths would need years to prepare such a mass of jewels, there must have been goldsmiths in Mycenæ from whom such jewels could have been bought ready-made. He spoke of the necklaces, too, and of the golden mask taken from one of the bodies, which must evidently be a portraiture of the deceased. Dr. Schliemann then proceeded to show that in a remote antiquity it was either the custom, or, at least, that it was nothing unusual that living persons wore masks. That also immortal gods wore masks was proved by the bust of Pallas Athenè, of which one copy was in the British Museum and two in Athens. It was also represented on the Corinthian medals. The treasures of Mycenæ did not contain an object which represented a trace of Oriental or Egyptian influences, and they proved, therefore, that ages before the epoch of Pericles there existed here a flourishing school of domestic artists, the formation and development of which must have occupied a great number of centuries. They further proved that Homer had lived in Mycenæ's golden age, and at or near the time of the tragic event by which the inmates of the five sepulchres lost their lives, because shortly after that event Mycenæ sank by a sudden political catastrophe to the condition of a poor powerless provincial town, from which it had never again emerged. They had the certainty that Mycenæ's flourishing school of art disappeared, together with its wealth; but its artistical genius survived the destruction, and when, in later centuries, circumstances became again favourable for its development, it lifted a second time its head to the heavens.

No doubt Dr. Schliemann's theories will be subjected to much criticism when the full details and drawings appear in his forthcoming work. Of the value of the discoveries themselves there can be but one opinion. Those alone which have been made in the Acropolis of what many have been inclined hitherto to regard as a half mythical city are of themselves sufficient to entitle him to an important place in the field of scientific research. Both to the historian and ethnologist his researches must prove of the greatest value, and all who have been stirred with the recital of the deeds of the Homeric heroes will rejoice to have henceforth reasonable external evidence for regarding them as something more than myths.

FERTILISATION OF FLOWERS BY INSECTS[1]

XVI.

Alpine Species of Gentiana adapted to Lepidoptera.

GENTIANA BAVARICA (Fig. 106-108), *G. verna* (Fig. 109-111), *G. nivalis* (Fig. 112-114), of which I have examined living specimens in the Alps, and *G. imbricata*, *æstiva*, *pumila*, and *utriculosa*, of which I have examined only dried specimens,[2] agree so completely in the structure of their flowers, and in their contrivances for cross-fertilisation by insects, that they all obviously belong to the same section of the genus Gentiana, and are adapted to the same group of visiting insects. They all possess a long narrow corolla (Fig. 106),[3] which contains in its lowermost portion the honey, secreted, as in our

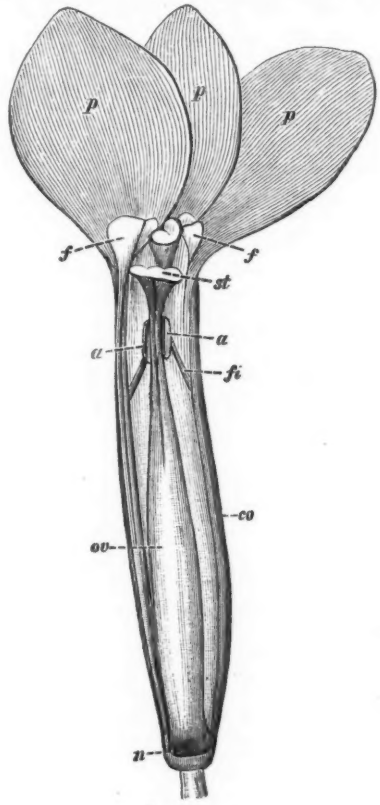


FIG. 106.

FIGS. 106-108.—*Gentiana bavarica*. FIG. 106.—Lateral view of a flower which begins twisting and closing, the calyx and the anterior portion of the corolla having been removed (3½ : 1). FIG. 107.—The same flower viewed from above (3½ : 1). The margin of the circular stigma is already covered by the revolving corolla. FIG. 108.—The stigma seen from above (7 : 1).[4]

two first groups, by an annular swelling at the base of the pistil[5] (*n*, Fig. 106, 111, 113). They all have the entrance to the corolla-tube closed by the bi-lobed stigma being dilated into a circular disc (*st*. Fig. 106, 109, 112), and bordered at its margins with hair-like papillæ (Fig. 108). In all of them small openings are visible in the fully-opened flower between the margin of the stigma and the inclosing corolla (Fig. 109); but as soon as the corolla begins twisting (Fig. 110), these openings are concealed. In all of them the anthers surround the stigma, thus placing their pollen on the way to the honey (*a*, Fig. 106, 110, 114). They can all, therefore, be fertilised only by such insects as have a proboscis sufficiently long to reach the base of the corolla, and at the same time either sufficiently slender to enter through the small openings (Lepidoptera), or sufficiently strong forcibly to enlarge the entrance of the flower (humble-bees). Such an enlargement, indeed, would be possible by the expansion of the same folds between the petals (*f*. Fig. 106, 107, 109, 110, 112), by which the flower is enabled to twist, and to close, as soon as colder weather frightens away its natural fertilisers.

Now, looking about to discover what Lepidoptera and humble-bees might be the natural fertilisers of the present

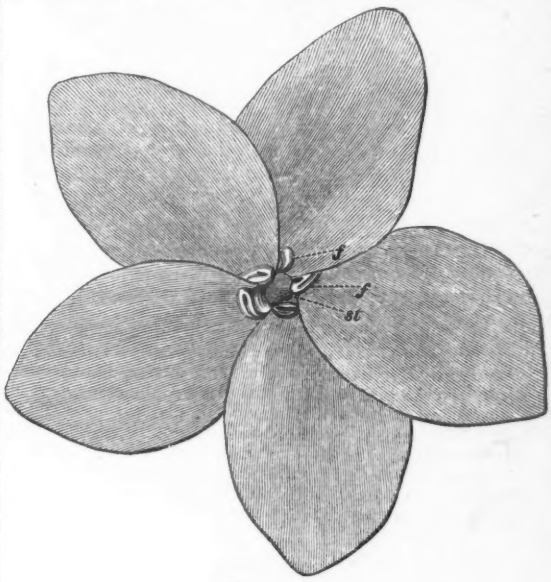


FIG 107.

group, I was surprised at the fact that of all the 162 species of Lepidoptera which I had observed visiting flowers in the Alps, *Macroglossa stellatarum* alone has a proboscis sufficiently long (25-28 mm.) to reach the honey in all the species of Gentiana in question, and that of the Alpine humble-bees, even those provided with the longest proboscis of all, *Bombus hortorum* (18-21 mm.) and *B. opulentus*, Gerst. (22 mm.), are incapable of reaching the honey in all the above-named species, except by thrusting the whole of their head into the narrow corolla-tube. Moreover, direct observation seems to prove that humble-bees insert at the most their proboscis into these flowers, but never their whole head. For instance, in the Albula Pass, July 28, 1876, I saw a humble-bee flying about for a long time in search of flowers; at length it hastily visited *Gentiana verna*, but having only once thrust its proboscis into a flower, it flew away out of my sight. Likewise near Pontresina, August 4, 1876, I saw *Gentiana nivalis* visited by *Bombus mendax*, Gerst. ♀, but, after having hastily tried one or two flowers in the same

FIG. 108.

[1] Continued from p. 319.

[2] For specimens of *G. pumila*, *æstiva*, *prostrata*, *Frælichii*, and *purpurea*, I am indebted to Prof. Ascherson of Berlin.

[3] Only in *G. æstiva* the corolla-tube seems to be considerably wider than in the other species.

[4] The following explanation of the lettering applies to all the figures:—*a* = anthers, *co* = corolla, *fi* = filaments, *n* = nectary, *o* = openings conducting to the honey, *ov* = ovary, *p* = petals, *s* = sepals, *st* = stigma, *f* = folds of the corolla by which its twisting is made possible.

[5] I found the length of the corolla-tube, from the nectary to the stigma, in the species in question as follows: *G. æstiva*, 26-28 mm; *G. verna* and var. *brachyphylla*, 23; *G. bavarica*, 20-22; *G. utriculosa*, 18-22; *G. pumila*, 16-18; *G. imbricata*, 15; *G. nivalis*, 13-16 mm.

manner, it passed over to *Trifolium nivale*, which it then sucked perseveringly. If from these observations we may infer that the forcible enlargement of the narrow corolla-tube of the species of Gentiana in question is too inconvenient for the humble bees, then the only insects capable of gaining the honey in all the species of the present group and capable of regularly visiting and cross-fertilising them, are *Macroglossa stellatarum*, and perhaps some other Sphingidæ not yet observed by myself in the

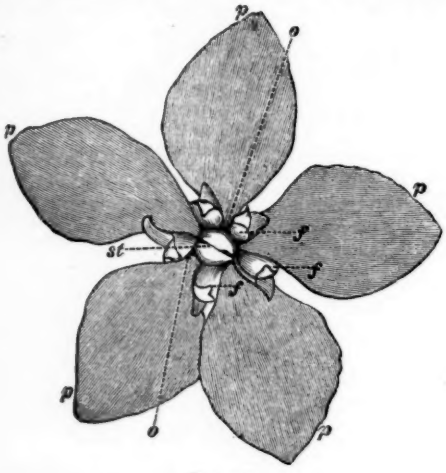


Fig. 109.

Figs. 109-111.—*Gentiana verna* (3/4 : 1). Fig. 109.—Flower, completely opened, seen from above. Fig. 110.—Upper portion of the same flower, bisected longitudinally, showing the pistil and the anthers. Fig. 111.—Lowermost portion of the same flower, showing the nectary.

Alps. The smallest of the species in question, *G. nivalis* (13-16 mm.), *G. imbricata* (15 mm.), and *G. pumila* (16-18 mm.), may also be fertilised by some moths[1] and by many butterflies[2] which will all easily insert their slender proboscis into one of the small openings at the base of the corolla-tube, then, withdrawing it smeared with pollen, will leave some pollen-grains on the margin of the stigmatic disc, and, when inserting the proboscis into another flower, will effect its cross-fertilisation by stripping off some of the pollen-grains from the hair-like papillæ at the margin of

Fig. 110.

Fig. 111.

the stigmatic discs. Humble-bees, on the contrary, at all events, seem to be of very small or no importance as fertilisers of this group. If thus, by indirect inference, we are led to the conclusion that the present group is exclusively adapted to Lepidoptera, we ought to embrace this opinion with due precaution; for all visits of Lepidoptera, hitherto directly observed by the comparison of the length of their proboscis with the length of the corolla-tube, prove to have been only fruitless attempts. On *G. nivalis* (corolla-tube 13-16 mm.), near Pontresina, August 4, 1876, I observed *Cænonympha satyrion*, Esp. (proboscis, 7 mm.), and *Hesperia serratulæ*, Rbr. (10-11 mm.); on *G. verna* (corolla-tube, 23 mm.) in the Albula Pass, July 28, 1876, I found *Melitæa aurinia*, var. *Merope*, Prunn. (7 mm.), *Argynnis pales*, S. V. (9-10 mm.), and *Erebia lappona*, Esp. (8-9 mm.); on *G. bavarica* (20-22 mm.), Kerner[1] observed *Agrotis cuprea* (12 mm.), I myself, upon the Piz Umbrail, July 15, 1875, saw *Erebia lappona*, Esp. (8-9 mm.); in the Albula Pass, July 27, 1876, *E. lappona* and *Melitæa asteria* (5-6 mm.); in the Val del Fain, August 5, 1876, *Melitæa aurinia*, var. *Merope*, Prunn. (7 mm.), all easily inserting their proboscis into the corolla-tubes, but all apparently without any advantage to themselves, though, by their repeated fruitless attempts, some cross-fertilisation of the flowers may have been effected. No direct observation

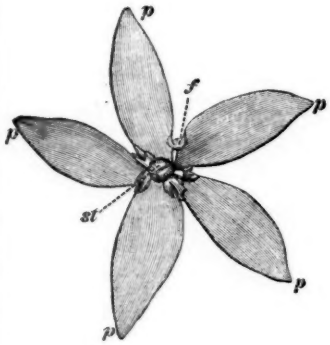


Fig. 112.

Fig. 113.

Figs. 112-114.—*Gentiana nivalis* (3/4 : 1). Fig. 112.—Flower seen from above. Fig. 113.—Pistil. Fig. 114.—Upper portion of the flower bisected longitudinally, showing one of the anthers (*a'*) in contact with the margins of the stigma.

of the true natural fertilisers of the species of Gentiana belonging to our fourth group has yet been made.

In former articles I have shown that frequently, of different species of the same genus, those possessing the most conspicuous flowers are adapted to cross-fertilisation by insects, whilst other species of the same genus, possessing less conspicuous flowers, have recourse to self-fertilisation, in case the visits of insects are wanting. This statement is also confirmed by the species of Gentiana in question. For in *G. verna* and *bavarica*, differing from *nivalis* not only by the considerably larger size of the separate flowers, but also by a number of flowers which stand close together, thus being easily seen from a great distance, the possibility of self-fertilisation is excluded by the position of the stigma and the anthers (as shown by Fig. 106 and 110); whereas in *Gentiana nivalis*, whose flowers are much smaller and more distant one from another, one or some of the anthers commonly come into contact with the margin of the stigma (as shown by Fig. 114), and effect self-fertilisation in case cross-fertilisation by insects is wanting.

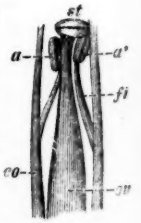


Fig. 114.

[1] *Plusia gamma*, 15-16 mm.; *P. interrogationis*, 15; *P. Hochenwarti*, 13.

[2] *Argynnis adippe*, 13-14; *A. aglaia*, 15-18; *A. niobe*, var. *eris*, 13-16; *A. paphia*, 12-14; *Colias phicomone*, 13-14; *Erebia goante*, 11-14; *Hesperia sylvanus*, 16; *Papilio machaon*, 18-20; *Pararge maera*, 13-14; *Parnassius apollo*, 12-13; *P. delius*, 11-16; *Pieris brassicæ*, 15-16; *P. cratægi*, 15; *P. rapæ*, 14-18; *Vanessa cardui*, 15; and *V. urticæ*, 14-15 mm.

[1] Kerner, "Schutzmittel der Blüthen gegen unberufene Gäste." (Wien 1876).

The most striking peculiarity of our fourth group of Gentiana is the sensibility of their flowers to the influence of the weather, which is apparently connected with the sensibility of their natural fertilisers to the same influence. The following observation clearly shows that different species differ widely in this sensibility, even when growing in the same locality. On the Piz Umbrail, July 16, 1874, I had collected some plants of *Gentiana bavarica*, var. *imbricata*, and of *G. verna*, and put them in my sleeping-room in the Quarta Cantoniera, upon a plate filled with water. The next morning, at half-past four o'clock, I found the flowers of *G. bavarica* already opened, those of *G. verna* still closed. I placed the plate outside the window, where the intensity of light was at least as great, but the temperature much lower, and all the opened flowers began twisting. After they had closed, I brought them back into the room, and they opened again. Repeating this trial from half-past four to half-past six o'clock, I saw them two or three times closing and opening again. *Gentiana verna*, standing upon the same plate, during this time, had not yet opened a single flower.

From this observation, the further prosecution of which was prevented by my departure, it is obvious (1) that the opening of the flowers of these species of Gentiana is caused

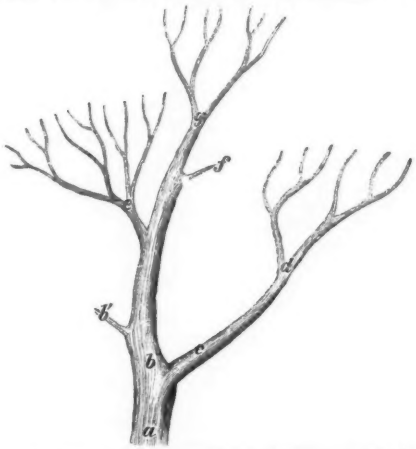


FIG. 115.—Pedigree of the species of Gentiana of Germany and Switzerland.

by heat, not by light; (2) that *G. verna* requires a higher temperature for opening than *G. bavarica*, var. *imbricata*. Possibly this is one of the causes which makes *G. verna* descend into sub-Alpine and low lands, whilst *G. bavarica* is confined to the Alpine region.

Comparing the present group with the foregoing ones, we need hardly doubt that it is most nearly allied to our second group, from which it differs only by the narrowness of the corolla, by the further development of the folds between the petals, and in connection with this by their greater sensibility, and by the lobes of the stigma being dilated. *Gentiana prostrata*, agreeing in every other respect with our fourth group, has as yet retained the two twisted stigma-branches, and therefore may be considered as a connecting species between the second group and the fourth, which is descended from it.

Summing up the above relations between the species of Gentiana of Germany and Switzerland, we obtain a pedigree like Fig. 115, in which the signification of the lettering is as follows:—

(*a*) Hypothetical ancestral form with fully open flowers, twisted stigma-branches, diverging stamens, and honey secreted at the base of the flower in the angle between the base of the pistil and the corolla. From this ancestral form we see two branches *b* and *c* descend; *b* with the nectary confined to the base of the pistil, *c* with nectaries at the base of the corolla. From the branch *c* has developed the sub-genus *Entotricha* Frœlich (*d*), containing six species:—*G. campestris*, *germanica*, *amarella*, *obtusifolia*, *tenella*, and *nana*, adapted both to Apidæ and to Lepidoptera. Of the branch *b* an original form, *G. lutea* (*b'*), has been preserved, accessible to insects of all orders, but from the same branch has descended the large sub-genus *Cœlanthe* Frœlich (*e*), containing eleven species:—*G. punctata*, *pannonica*, *purpurea*, *cruciata*, *asclepiadea*, *Pneumonanthe*, *Frœlichii*, *frigida*, *acaulis*, *excisa*, and *ciliata*, all adapted to humblebees. One branch of this sub-genus (*e*), by narrowing the corolla, perfecting the folds between the petals, dilating the stigma-branches, and thus adapting the flowers to Lepidoptera, has further developed the sub-genus *Cyclostigma* (*g*), containing seven species:—*G. bavarica*, *verna*, *æstiva*, *imbricata*, *pumila*, *utriculosa*, and *nivalis*. As a link between the ancestral sub-genus (*e*) and the derived subgenus G., has been preserved *G. prostrata* (*f*).

Lippstadt HERMANN MÜLLER

RECENTLY PROPOSED IMPROVEMENTS IN MUSICAL INTONATION

THE harshness of the present system of tuning has been a source of constant complaint since it was first introduced, about a century and a half ago. But of late years several more or less practical attempts have been made to overcome this defect without interfering with the quality of our musical tones. Instruments with fixed tones, as the organ, piano, and harmonium, lead voices, and the inalterable quality of vocal tone has therefore to be constantly kept in view. The instruments exhibited in the Loan Collection of Scientific Apparatus at South Kensington are enough to show both the objects aimed at and the nature of the mechanical appliances by which it is hoped they may be more or less reached. It is difficult to give an intelligible account of them within the compass of an article, but Dr. Stone's two lectures[1] and Mr. Bosanquet's more recent work[2] will supply details and figures.

On examining musical tones generally,[3] we are led to the conclusion that the first requisite is to have a succession of notes forming perfect Octaves, Fifths, and major Thirds, that is, making numbers of vibrations which the air executes during the same length of time in the ratios 1 : 2, 2 : 3, and 4 : 5 respectively. An examination of prevailing systems of modulation, has shown[4] that a strict fulfilment of this condition would require 117 notes to the Octave—a mechanical impossibility on any instrument with fixed tones. Such a scheme must, however, be made the basis of subsequent work. Moreover if combinations with what is called the harmonic Seventh or 6 : 7 be admitted, then we should require very nearly to double the above number of separate notes. It follows, therefore, that we must either restrict our desires of modulation (which is not likely to happen) or be content to use more or less imperfect intervals, and the question turns upon the degree of endurable imperfection. It must be remembered that these apparently innumerable delicacies of sound present no real difficulty to the singer or violinist when he once knows the theory on which they have to be produced, for they are all generated by extremely simple intervals. The difficulty, indeed, is to avoid them, especially in part music, and to put up with alterations, apparently arbitrary and certainly neither

1 "Sound and Music," by Dr. W. H. Stone, in the series of Science Lectures at South Kensington, 1876, pp. 46. (Macmillan.)

2 "An Elementary Treatise on Musical Intervals and Temperament, with an Account of the Enharmonic Harmonium Exhibited in the Loan Collection of Scientific Instruments, South Kensington, 1876; also of an Enharmonic Organ Exhibited to the Musical Associat on of London, May, 1875," pp. 94, 1876. (Macmillan.)

3 For reasons and details see Helmholtz's "Sensations of Tone."

4 See my translation of Helmholtz, pp. 669-672.

easy to conceive nor to produce. No *temperament*, as such a makeshift intonation is called, could exist except by help of an instrument with fixed tones, and the most practised tuner finds it impossible to produce a satisfactory result, except by mechanical means.[1] How then could we expect a singer to produce at will any of the fifty or more schemes of tuning that have been invented, or even either of the only two that have been a success, the mean-tone and the equal temperaments?[2]

The Greek system of intonation, as we know from Euclid's "Section of the Canon," consisted of a series of perfect Fifths, and may for convenience be represented by F, C, G, D, A, E, B, where it will be found that each note is a Fifth higher than the preceding. It is therefore the simplest and most intelligible that can be imagined, and is in fact at the base of all music. Reduced to the same Octave and played as a major scale this gives C, D, E, F, G, A, B, C. This is perfectly singable, and produces excellent melodic effects. But if we attempt to play the usual major and minor chords of the scale with these notes, as F A C, C E G, G B D, D F A, A C E, E G B, the effect, as I know by frequent experience, is simply hideous, so that there is no difficulty in understanding why the Greeks called Thirds "dissonances" (*diaphōna*), and had no harmony. Observing that in the series of Fifths E is the fourth Fifth above C, we may therefore say that the major Third cannot be identified with the fourth Fifth up, reduced to the same Octave. On calculating out the ratio we find $C : E = 4 : 5 \times \frac{81}{80}$, whereas the real consonance requires 4 : 5. This would be produced by taking a note E_1, as we may write it, which is flatter than E in the ratio of 80 : 81. The result of carrying this out to the extent of modern modulation is the series of 117 notes to the Octave already mentioned.

In the Loan Collection, one instrument, General Perronet Thompson's enharmonic organ, grappled with this problem to the extent of forty notes.[3] But its three finger-boards with occasional extraordinary shapes and colours to the finger keys (figured by Dr. Stone, *op. cit.*, p. 32) might well frighten the uninitiated. Yet General Thompson, himself unable to play, taught a blind organist how to use it, so that in a fortnight she could perform in public, and I have often heard the instrument played by others, who did not complain of any difficulty. Helmholtz (in my translation, p. 636), who also heard it played, speaks of its chords as "extraordinarily harmonious," but the quality of tone (one stop of "metal principals") did not distinguish the consonances effectually at all times, and the compass of forty tones of course materially limited modulation except into tonic and relative minors, which were well provided for.

Mr. Colin Brown has also grappled with perfect tertian harmony, but has exhibited only a model of his key-board, figured and described by himself in Dr. Stone's book (*ib.* pp. 42-45).[1] His scheme allowed of sufficient modulation into the dominant and sub-dominant major keys, and their first relative minors, but almost utterly ignored the tonic minor, and further minor modulation. As the instrument is an harmonium, the quality of tone is remarkably suitable for bringing out the effects of perfect Fifths and Thirds, and when proper music was selected, the result was most satisfactory. The keyboard is much simpler than General Thompson's, and has the great advantage of being the same in all keys. Such an instrument is of the highest value for lecture illustrations of harmony, and for training of vocalists in perfect intonation. As an independent instrument, its power of modulation is too limited, and the fingering usually simple enough, occasionally becomes very troublesome.[2]

These are the only instruments on which perfect tertian harmony was attempted. In the others some sort of compromise was come to. Mr. Guéroult showed his modification of Helmholtz's double keyboard, each finger-key being cut in half, and the upper half giving, generally, a note flatter than the lower half by a comma.[3]

This is a most convenient instrument for scientific purposes, but from its very limited capacity not so well suited as even Mr. Colin Brown's for playing musical pieces. The fingering is also full of difficulty, having all the imperfections of the ordinary board, with many others superadded, and differs in every key.

We have seen that if we use the fourth Fifth up from any note, when reduced to the same Octave, as the major Third, it is too sharp by a comma, and unbearably dissonant, but if we use the eighth Fifth down, also reduced to the same Octave, the result is a note just $\frac{1}{11}$ comma too flat, which is very much closer than the major Third in actual use (for that is $\frac{7}{11}$ comma too sharp), and is not at all disagreeable, although not by any means as pleasant as the perfect major Third. Thus, going down eight perfect Fifths from C, we get in succession F, B*fl.*, E*fl.*, A*fl.*, D*fl.*, G*fl.*, C*fl.*, and F*fl.*, and the proposal is to use F*fl.* for E_1. On the piano, and all instruments in the usual temperament, these are the same notes, but they are not so when thus tuned, and they are still strictly distinguished in our usual musical notation. Herr Georg Appunn, to whom we are indebted for various excellent acoustical apparatus, and especially for a tonometer (exhibited in the Loan Collection), which is self-verifying, and enables us to measure pitch with wonderful accuracy, likewise showed an harmonium, consisting of three rows of keys, the upper ones in the form of studs, with practically the usual fingering, consisting of thirty-six tones

[1] "Mr Ellis has given a practical rule," [for producing the usual, intentionally equal, temperament], "which does not err in its results by much more than the hundredth part of a semitone" (Seldom as much as that.) "It is—make all the Fifths which lie entirely within the Octave c′ c″ [middle C to the C above] beat *once* per second; and make those which have their upper notes above c″ beat *three times* in *two* seconds. Keeping the Fifth f′—c″ to the last, it should beat once in between one and two seconds. See Ellis's "Helmholtz," p. 785. This is a perfectly practicable rule, and tuners ought to be instructed in the use of it. *There are few tuners who can produce a tolerable equal temperament.*"—Bosanquet, *op. cit.*, p. 5.

[2] See my paper on Temperament (*Proc. Roy. Soc.*, vol. xiii., pp. 404-422), where more than fifty schemes are calculated and analysed. Mr. Bosanquet has a most interesting chapter on the history of the mean-tone temperament or old organ tuning, the only one known to Handel, and its complete realisation, without its former "wolves," by means of his own fingerboard, pp. 24-40. He rightly considers this temperament most suitable for the organ (p. 58) as the equal temperament is for the pianoforte. But then the voices of a choir might be led by the completed mean-tone system without much injury to the chords, which are shivered by the equal temperament.

[3] These are given by Mr. Bosanquet, *op. cit.*, p. 22, arranged according to his own finger-board, which completely does away with the terrors of the original, but also expressed in his own notation, which implies a temperament which General Thompson well knew and repudiated. Hence I add them here in my own symmetrical arrangement (*Proc. Roy. Soc.*, December, 1874, vol. xxiii. p. 29, called "the simplest" by Mr. Bosanquet, p 50), in which the columns represent ascending Fifths, and the lines from left to right ascending major Thirds, and where the superior and inferior numbers indicate sharpening or flattening by a comma (this is here published for the first time).

General P. Thompson's Enharmonic System:—

c^1	e			
f^1	a	c_1 *sh.*		
b^1 *fl.*	d	f_1 *sh.*	a_2 *sh.*	
e^1 *fl.*	g	b_1	d_2 *sh.*	f_3 *ssh.*
a^1 *fl.*	C	e_1	g_2 *sh.*	b_3 *sh.*
d^1 *fl.*	f	a_1	c_2 *sh.*	e_3 *sh.*
g^1 *fl.*	b *fl.*	d_1	f_2 *sh.*	a_3 *sh.*
	e *fl.*	g_1	b_2	d_3 *sh.*
	a *fl.*	c_1	e_2	g_3 *sh.*
		f_1	a_2	c_3 *sh.*

[1] The notes used in the figure are comprised in the second, third, and fourth columns of the above scheme of General P. Thompson's, but carried further, the second column rising to D *sh.* and descending to F *fl.*, the third rising to B_1 *sh.*, and descending to A_2 *fl.*, and the fourth rising to A_3 *ssh.*, and descending to G_3, giving fifty-two notes in all; but the instrument performed on at Dr. Stone's lecture (July 25, 1876) had not so large a compass.

[2] "It is clear that this (Mr. C. Brown's) arrangement adapts itself with some facility to all music in which there is not much modulation, or in which the modulation is of a simple type. It is, however, easy to give instances which will at once involve the performer in difficulties." Bosanquet, *op. cit.* 48-9. Mr. Bosanquet, who is a practised organist, having studied the fingering, and played on the instrument at the Glasgow Meeting of the British Association, is well able to speak to its capabilities.

[3] Disregarding one very slight alteration, the compass of twenty-four notes extended from E *fl.* to B in the second column of General Thompson's, from G_1 to G_1 *sh.* in the Third, and from F_2 *sh.* to B_2 *sh.* in the Fourth. With the exception of Two Fifths (G_1 *sh.* to E *fl.*, taken to be D_1 *sh.*; and B_2 *sh.* to G_{11}, taken at F_2 *ssh.*), which were too flat by $\frac{1}{11}$ of a comma, as all Fifths ought to be on the piano, this gave a complete succession of dominant modulations, but only admitted of five minor keys.

forming thirty-five perfect Fifths, which, by the contrivance of using the eighth Fifth down as the major Third, into the place of which its finger-key is placed, gives very free play for modulation in all directions with perfect uniformity, although of course slightly imperfect intonation.[1]

To this system of perfect Fifths and major Thirds identified with eight Fifths down, we must attach all the other contrivances for reducing the number of notes necessary to tertian harmony, without seriously offending the ear. The practicability of arranging any number of dozens of such notes in the Octave, up to at least seven dozen, so that they should be entirely under the command of the performer, be fingered precisely in the same way in all keys, and have a style of fingering which is of about the same difficulty as that for three sharps or three flats on the piano, has been proved to demonstration by the "generalised keyboard" of Mr. Bosanquet, exhibited at the Loan Collection, for his enharmonic harmonium, and, up to four dozen finger keys to the Octave, to the Musical Association upon his enharmonic Organ.[2] This keyboard is quite a triumph of ingenious construction, founded on rigorously scientific principles, for the practical solution of an apparently insoluble problem.

The modifications of the perfect Fifth system (to which Mr. Bosanquet seems much inclined, *op. cit.*, p. 57) depend on the discovery that fifty-three perfect Fifths exceed thirty-one Octaves by only about $\frac{36}{1000}$ of an equal semitone, or very nearly $\frac{2}{11}$ of a comma. Helmholtz proposed to reduce every Fifth by $\frac{1}{53}$ of a comma. This would make fifty-three of the flattened Fifths to be about $\frac{1}{10}$ of a semitone less than thirty-one octaves, too large an interval for good ears not to perceive, being nearly half a comma, but then all his Fifths would be audibly perfect, and all his major Thirds absolutely perfect. Mr. Bosanquet endeavoured to tune a stop on his enharmonic organ in that way, but the effect with stopped pipes did not repay the immense trouble of tuning (*ibid.*), which cannot be truly effected without much mechanical assistance, and is therefore generally impracticable.

The great difficulty of tuning is also an objection which applies to Mr. Bosanquet's own proposal to divide the Octave absolutely into fifty-three parts (see *op. cit.*, p. 56). This would flatten the Fifth still less, but of course would also make the major Thirds nearly as flat as those in the system of perfect Fifths, from which his differ only by about $\frac{1}{44}$ of a comma. It is not likely that Mr. Bosanquet has been able to tune to such a degree of accuracy. And as the object of the division of fifty-three is only to modulate *ad infinitum*, such accuracy is needless for general purposes, for which forty-eight perfect Fifths (or, as I believe, six sets of eight perfect Fifths, differing by perfect major Thirds from each other, and hence comparatively easy to tune by Fifths and check by Thirds), would fully suffice.[3]

[1] The notes may be considered to be those in Gen. Thompson's second column continued up to *Ash.* and down to *Affff.* (or A quadruple flat), the names being altered to those of the finger-keys corresponding to those on the ordinary piano, so that no more sharps and flats are used than in ordinary notation. Unfortunately the bellows and some parts of the mechanism were injured in the carriage, and hence the full effect could not be appreciated. There were two extra rows of keys to bring out Herr Appunn's favourite minor Third, 16 : 19, which is slightly closer than that on the piano, and very effective in certain cases, but the fingering for these was new and difficult, and could not be considered practical.

[2] The first, with eighty-four notes to the Octave, is figured in Mr. Bosanquet's book (*op. cit.*, p. 23), where it is fully described, and is also explained at length in my Helmholtz, pp. 692–696. In both books the builder's name and address are given, with his prices for compasses of two to seven dozen keys per Octave. Such instruments are indispensable for the scientific cultivation of music. It is also suited for mean-tone intonation.

[3] See the full explanations in my Helmholtz, pp. 656, 657, 783. Tuning by perfect intervals is the only system practicable without mechanical assistance. But even to tune the thirty-five Fifths of Appunn is impossible by ear alone. I find tuners have a difficulty with only eight notes, forming seven successive Fifths. But when the next eight notes are taken as major Thirds to these, all verified by forming Fifths with each other and producing correct differential tones with the original set, we may hope for some correctness. The only really satisfactory way of tuning is by calculating the pitch of each note, and then causing a set of forks, or Appunn reeds, to be constructed, each too flat by four vibrations, which can be done exactly by Appunn's tonometer. After tuning a note roughly to one of these, sharpen it till it beats four times in a second with the standard. Any temperament, even Helmholtz's, the best in existence, can thus be easily and perfectly realised note by note.

The instruments exhibited in the Loan Exhibition and the others indicated in the preceding lines may, therefore, be said to have practically solved the difficulty of tertian harmony on instruments with fixed tones, and they have even approached to a solution for septimal harmony (which uses the harmonic Seventh ; see Bosanquet, *op. cit.*, p. 41). Voices in part music, when unaccompanied, must sing in just tertian or even septimal harmony, but when accompanied they will inevitably follow the instrument. Violinists can do what they like, but are too much inclined to Greek intonation, which is all very well by itself, but which the performer should learn to modify by something better than a rule of finger, in double-stopped passages and part music. With the bass the comma stop may be made effective to a great extent, and Dr. Stone is trying what he can do with the oboe and clarinet (*op. cit.*, p. 35), so that there are some hopes of improving even the orchestra. Enough, at least, has been done on the instruments mentioned and in the practice and system of study of the Tonic Solfaists (Helmholtz, p. 640) to show that it is practically possible greatly to improve musical intonation.

ALEXANDER J. ELLIS

ON PHOTO-CHEMICAL PROCESSES IN THE RETINA

IN an article which lately appeared in NATURE (vol. xv., p. 308), I gave an account of certain very remarkable discoveries made by Prof. Kühne, of Heidelberg, which added additional interest to the startling announcement contained in a recent communication made by Prof. Boll, of Rome, to the Berlin Academy, to wit, that the external layer of the retina is, during life, of a purple colour, which disappears at death, but which is, during life, continually being bleached by the action of light. In my first communication I stated that the account of Boll's researches which I was able to give, was only quoted at second-hand from Kühne's paper, as the number of the *Proceedings* of the Berlin Academy containing Boll's communication[1] had not yet reached Manchester. Having now had the opportunity of reading that communication, I am able to state that the summary of it contained in my first article was correct in every particular. As the paper is, however, one of peculiar importance I propose, with the concurrence of the Editor of NATURE, to insert a *verbatim* translation of it in next week's number of NATURE.

It is with great surprise that I have heard that the prominence given to Prof. Kühne's researches on the "Vision-purple" in my article in NATURE has given some pain to Prof. Boll, who probably feels some disappointment in not having been allowed to remain in sole possession of the promising field of research upon which he had entered. It is with still greater surprise, however, that I have read the remarks which D^r. Warlemont, editor of the *Annales d'Oculistique*, has added to the literal translation of my article which he has published in that journal,[2] and which follows a brief abstract of Boll's paper.

"Nous appelons toute l'attention de nos lecteurs sur les deux articles qu'ils viennent de lire, et qui signalent une découverte propre à révolutionner la physiologie de la rétine, à renverser quelques unes des idées reçues, à en affirmer beaucoup d'autres. *Tout le mérite de la découverte de la coloration propre de la réline appartient à M. le professeur Boll, avec toutes ses conséquences*, dont M. Kühne nous parait s'être prématurément emparé. M.

[1] Monatsbericht der königlichen preussischen Akademie der Wissenschaften zu Berlin, November, 1876. Gesammtsitzung vom 23 November, 1876, S. 783–787.

[2] *Annales d'Oculistique*, tome lxxvii., Janvier-Février 1877, pp. 78-81.

Boll avait évidemment entrevu toutes ces conséquences, et il eut été de bon gout, nous semble-t-il, de lui laisser le temps de les dérouler à l'aise. C'est donc sans droit que nous voyons déjà, dès à présent, la presse parler, à propos de ce fait, des 'découvertes de MM. Boll et Kühne' et le nom de ce dernier associé a celui du *seul inventeur*.

"Deux gamins suivaient un trottoir; l'un d'eux sifflaient un air, dont il n'était qu'à la moitié, quand le second se mit à la continuer: 'Une autre fois,' lui dit le premier le regardant très mécontent, 'tu voudras bien commencer toi-même.'"

If I quote the above sentences it is to show that they are as much opposed to truth as they are to the interests of science, or as they are repugnant to good taste. When a scientific man has published a discovery it is to the interest of the scientific world that all who will or can should be at liberty to repeat the experiments or observations which led to it; if other great discoveries are made by the new labourers it is to the interest of science that they be published.

In the particular case in point it would appear that Prof. Boll re-discovered (and, what is more, *appreciated the full value* of) a fact which had really been observed by some others (Leydig in 1857, and Max Schultze in 1866), but which had certainly not become part and parcel of the common stock of scientific knowledge, viz., that the rods of the retina are red, he observed that under the circumstances of his own experiments the colour faded at death, and arrived at the false conclusion that the colour was a function of the vital condition of the retina. He, however, observed the remarkable action of light in modifying the colour. "During life," he announced in his paper, "the peculiar colour of the retina is continually being destroyed by the light which penetrates the eye. Diffuse daylight causes the purple tint of the retina to pale. The more prolonged, dazzling action of the direct rays of the sun entirely destroys the colour of the retina. In darkness the intense purple colour is again restored. This objective alteration of the peripheral structures of the retina brought about by the rays of light undoubtedly occurs in the act of vision." This was the great discovery which Boll made, and with which his name will ever remain honourably connected. Although, however, he had been in full possession of the facts in the month of June of last year, when he demonstrated them to Professors Du Bois-Reymond and Helmholtz, and only published his paper in November, he did not succeed in making the discoveries with which, justly, the name of Kühne is now associated. Kühne showed that if the retinal purple is usually destroyed at death, the result is attributable to the action of light, persistence of the colour being by no means necessarily connected with the living condition of the retina. In his beautiful and far-seeing discovery of the true function of the retinal epithelium cells as restorers of the vision purple, he was fortunate enough to make a discovery which it would be very bold for any one—even for Prof. Boll—to say he would have made, had time and opportunities been granted. In saying that Boll had discovered everything referring to the vision purple, M. Warlemont shows that he has not appreciated the fact that two great discoveries have been made, the second supplementing the first, and actually needed in order that the significance of the first should be appreciated.

But I trust that the readers of NATURE do not think that I wish to depreciate the researches of Prof. Boll whilst I act as the champion of one who needs no champion, seeing that he illustrates in himself the truth of the adage, "le grand mérite est toujours probe."

Prof. Boll must reflect that great discoveries are rarely completed by one man, and that it is no shame, and should be no cause of sorrow, to the true man of science, if the conception which he has tried to develop and which he has almost raised to the position of a truth by his own work, receives its final development through the strivings of a fellow-worker.

Abandoning the polemical discussion upon which I felt myself almost compelled to enter, I would give an account of the most recent results obtained by Kühne on the "Vision Purple," and published by him in the *Centralblatt für die medicinischen Wissenschaften* for March 17 (No. 11).

The purple colour of the retina is now shown to depend upon the presence of a substance which can be dissolved and separated in the solid form. The only solvent of the vision-purple as yet known is bile, or a pure glyko-cholate. The filtered, clear solution of the vision-purple is of a beautiful carmine-red, which, when exposed to light, rapidly assumes a chamois colour, and then becomes colourless. As long as it is at all red the solution absorbs all the rays of the spectrum, from yellowish-green to violet, allowing but little of the violet, but all the yellow, orange, and red rays to pass. Accordingly, bloodless retinæ spread out and placed in the spectrum, between green and violet appear grey or black.

Kühne has exposed retinæ in different parts of a spectrum (obtained by allowing the sun's rays between eleven and one o'clock to fall through a slit 0·3 mm. wide upon a flint glass prism) in which Fraunhofer's lines were shown in great number and with great distinctness, and he has ascertained that in the yellowish green and green regions the vision-purple is bleached most rapidly; the action is less in the bluish green, blue, indigo, and violet; it is still perceptible in the orange and yellow, but not in the red or ultra-violet regions.

March 24 ARTHUR GAMGEE

OUR ASTRONOMICAL COLUMN

THE CAPE ASTRONOMICAL RESULTS, 1871–1873.—Mr. Stone has just circulated the results of meridional observations of stars made at the Royal Observatory, Cape of Good Hope, in the years 1871-1873. His present object has been not so much to furnish extremely accurate places of principal southern stars as to supply reliable positions of stars down to the seventh magnitude within 15° of the South Pole, and it is considered that this volume contains all Lacaille's stars in this region of the sky, and very nearly all sevenths not observed by him. It is the "first published instalment of the materials collected for the projected Catalogue." The separate results for mean R.A. and N.P.D. are given, with catalogues of places for the commencement of each year, the whole number of stars observed being about 1,400. Bessel's reduction constants are appended. This form of publication is perhaps sufficiently ample in the present day, though Mr. Stone alludes to a desire expressed by some astronomers to see the Cape observations printed in detail in the same manner as the Greenwich observations, a plan hardly practicable with the limited staff at his disposal, and which would involve very slow progress of the work with the resources of the Cape press. We are inclined to think that Mr. Stone exercises a wise discretion in limiting his volume to its present form, and thus assuring its comparatively early distribution in the astronomical world. As it is the volume is not produced without a considerable expenditure of time in the routine work of the reductions by the director himself.

VARIABLE STARS.—Mr. J. E. Gore, of Umballa, writes with reference to several stars which may prove to be variable:—(1) Lalande 14088 (Canis Major); this star was observed by Lalande, March 2, 1798, but the magnitude was not registered. It is marked of the ninth magnitude only on Harding's Atlas, but at the beginning of February in the present year Mr. Gore found it a little brighter than the sixth magnitude Lalande 14105, closely south of it, and "decidedly reddish." Argelander observed this star on December 23, 1852, and rated it 6 m. Heis

has not entered it. There appears a strong suspicion of variability in this case. The star's position for 1877 is in R.A. 7h. 8m. 11s., N.P.D., 112° 27′·8. (2) Harding has a star 6 m., a little south—preceding 40 Leonis Minoris, where in February last Mr. Gore found only a star of about 10 m. The position for 1800, reading off from the Atlas, was in about R.A. 157° 14′, N.P.D., 63° 28′. There is no star here in Lalande or Bessel, nor in the *Durchmusterung*. (3) About 1° 20′ south—following the 5 m. star 6 Canis Minoris, Harding has a 6 m. which on February 4 was only 7½ m., being less than Lalande 14720, but brighter than 14726; it was also less than the 7 m., about 30′ north-preceding, which is underlined in the Atlas. This star appears to have been observed as an 8 m. by Bessel (Weisse, VII., 780), and is called 8·1 m. in the *Durchmusterung*; Harding's place, however, requires a small correction in R.A. if Bessel's star is the one entered on his map.

BIELA'S COMET IN 1805.—Of the six observed returns of this comet that of 1805 was by far the most favourable for observation, and it approached very near to the earth as it sank below the horizon in Europe. At the beginning of December it exhibited a well-defined planetary disc, according to Huth, surrounded by nebulosity 20′ in diameter; on the 8th Olbers found it very distinct to the naked eye, and it remained visible without the telescope after the moon had risen, though at a south declination of 23°; the small well-defined nucleus which he had remarked in common with other observers he considered to be from twenty to thirty German miles in diameter. The comet was not observable in Europe after December 9, when it had reached 35½° S., but its after-course was a very favourable one for observation in the southern hemisphere, and, as Gauss remarked at the time, if observations from thence could have been obtained, it would have been practicable to determine at this appearance the true form of the comet's orbit, which, as is well known, greatly exercised the calculators of that day, and particularly Gauss and Bessel. The comet's apparent track in the southern heavens during the week subsequent to the cessation of observations in Europe was as follows, according to a computation from the definitive orbit in 1805, given by the late Prof. Hubbard:—

h. G.M.T.		R.A.	N.P.D.	Distance from earth.
Dec. 9	...	350 9	122 26	0·03681
10	...	344 49	134 38	0·03698
11	...	337 14	145 49	0·03893
12	...	326 3	154 53	0·04245
14	...	288 36	164 36	0·05276
16	...	250 5	164 23	0·06570

While referring to Biela's comet, it may be noted that if the period of revolution had been so lengthened in 1872, as to delay the perihelion passage until December 27, and thus bring the comet, or what remains of it, into close proximity to the earth on the night of the great meteoric display a month previous, its return in 1879 will take place under nearly the same circumstances as in 1832, when this body was the object of so much interest.

NOTES

WE understand that the Fullerian Professorship of Chemistry in the Royal Institution is likely soon to be vacant by the resignation of Dr. Gladstone.

THE Council of the Yorkshire College of Science, Leeds, have arranged to purchase for 13,000*l.*, the Beech Grove Hall Estate, comprising about three and a half acres, and situated a mile from the railway stations, and close to the Grammar School. The total donations to the College have now reached 42,456*l.*

It has been decided that of the statues of the two Humboldts which are to be erected in Berlin, that of Alexander will be given to Reinhold Begens to execute, and that of Wilhelm to M. P. Otto.

EVERY Thursday evening M. Leverrier receives at the Paris Observatory the provincial mayors who happen to be in Paris and explains to them the principles used by the International service for telegraphing its warnings all over France.

THE dreaded *Hemileia vastatrix* which has hitherto been confined to the coffee plantations of Ceylon and Southern India has at last made its appearance in Sumatra, and in all probability will find its way before long to the neighbouring islands where coffee is grown.

THE Settle Cave Exploration Committee have issued a circular asking further contributions to enable them to carry on their important work. The valuable contributions already made, both to the historical and prehistoric ethnology of Britain are already well known, but there is good reason to believe discoveries even more interesting than any yet described remain to be made. The Committee are now working in beds of still earlier age than those hitherto explored, and hope by perseverance to throw some light upon the condition of Britain and its inhabitants during some of the most obscure ages of its geological history, the interest and value of the explorations increasing as the work is carried down into lower and earlier beds. Except at the entrance, the rocky floor of the cavern has not yet been found, and it is impossible to say what treasures to science and aids to the unwritten history of man still lie beneath the feet of the explorers. Though liberally assisted by a grant from the British Association, the Committee find themselves obliged to appeal to the public for further funds, without which this interesting work will speedily come to a premature end. We are sure that the wants of the Committee only require to be made known in order to be supplied. The sum required is, after all, very moderate, and we hope that many who read this will send what they can afford to the Hon. Treasurer, Mr. John Birkbeck, jun., Craven Bank, Settle, Yorkshire.

THE present French University will probably be divided into seven or eight local universities—Paris, Nancy, Lyons, Bordeaux, Lille, Marseilles, Montpellier or Toulouse. The competition between the two last has been so sharp that it has been suggested to divide between them the boon sought for. None of the existing Faculties in a large number of provincial towns will be suppressed, but will become affiliated to the nearest university. Each university will be governed by a special Senate or Council, and the Minister of Public Instruction will have authority over all of them. Fellowships will be created by the government, and will be distributed according to merit, after due examination.

ALL who have read Mr. Smiles's "Scottish Naturalist" must remember the crowning incident of Mr. Edward's exhibition of his collection in Aberdeen, when, in his despair at the total unsuccess of his venture, he rushed to drown his misery in the Dee. Mr. Edward was the chief actor in a very different scene in the same city on Wednesday week, when the proverbially hard-headed and close-fisted citizens of Bon Accord tried to make amends for their former almost fatal neglect, by presenting him publicly, through their Lord Provost, with an olive-wood casket containing 333 sovereigns. Mr. Edward, who seems to be taking his sudden eminence very quietly, thanked the subscribers in a short speech spoken in broad Doric and characterised by perfect naturalness and much humour—Scotch, perhaps, but not Highland, as some of the papers characterise it, for Aberdeen and Banff are as much "Highland" as Berwick and Newcastle. Mr. Edward made no allusion to his former treatment by the certainly not obtuse Aberdonians, who, after all, can't be blamed for not making it their business to discover and succour genius, though the gift of the "bit boxie," as Edward called it, looks very like as if meant to be a peace-offering.

THE papers read at the Iron and Steel Institute last week were all of a purely technical nature. The Bessemer Medal was

presented by the President to Dr. Percy, who, in his reply, hinted that he is at present engaged in a large new metallurgical work. The Institute unanimously approved of the President's proposal to endeavour to obtain for the applied science societies a common permanent home. The Institute holds its autumnal meeting in Newcastle in September next, Norway having been abandoned, mainly, we believe, on account of the death of the Foreign Secretary, Mr. David Forbes. The Institute of Naval Architects, which also met last week in London, intends, we believe, to have for the first time an autumnal session, Glasgow and the Clyde having been selected for a visit next August.

THE following are the probable arrangements after Easter at the Royal Institution for the Friday evenings:—April 13, Dr. William Spottiswoode, Treas. R.S., Experiments with a Great Induction Coil; April 20, Mr. Frederick Pollock, M.A., Spinoza; April 27, Lieut.-Gen. Richard Strachey, R.E., F.R.S., The Physical Causes of Indian Famines; May 4, Rev. W. H. Dallinger, Researches on the Origin and Development of Minute and Low Forms of Life; May 11, Mr. D. Mackenzie Wallace, M.A., The Intellectual Movements and Secret Societies in Russia; May 18 [blank]; May 25, Mr. G. J. Romanes, The Evolution of Nerves and Nervo-Systems; June 1, Mr. Oscar Browning, The History of Education; June 8, Prof. Tyndall, F.R.S. The lecture arrangements are as follows:—Prof. J. H. Gladstone, F.R.S., Five Lectures on the Chemistry of the Heavenly Bodies, on Tuesdays, April 10 to May 15; Prof. Tyndall, F.R.S., Eight Lectures on Heat, on Thursdays, April 12 to May 31; Mr. Edward Dannreuther, Two Lectures on Chopin and Liszt, on Saturday, April 14, and Thursday, June 7; The Rev. A. H. Sayce, M.A., Three Lectures on Babylonian Literature, on Saturdays, April 21, 28, and May 5; Mr. Walter H. Pollock, M.A., Three Lectures on Modern French Poetry, on Saturdays, May 12, 19, 26; Mr. Charles T. Newton, C.B., Two Lectures on the Recent Discoveries at Mycenæ, on Saturdays, June 2 and 9.

THE first of the letters from Mr. Stanley, already referred to, is published in Monday's *Daily Telegraph*. It is dated Ujiji, August 7, 1876. Mr. Stanley has succeeded in circumnavigating Lake Tanganyika, exploring every indentation, and has made a material addition to our knowledge of this interesting body of water. As might have been expected, he has occasion to supplement and correct the observations of his predecessors. On the mistakes of the latter he dwells at quite unnecessary length, and discusses them in an aggravatingly apologetic tone, which becomes quite irritating, and does not in the least enhance the value of his own discoveries, which require no contrast to bring out their importance. The greater part of Mr. Stanley's letter is occupied with an account of his exploration of the Lukuga, Cameron's supposed outlet of the lake, but which Mr. Stanley maintains, on what appear solid grounds, to be merely a creek, the surface current of which is influenced by the wind. The most extraordinary result, however, of his examination of the lake and of the Lukuga, is that the former is rising with comparative rapidity—several feet since Cameron's visit—and that in the course of a very few years the Lukuga will develop into an effluent river, which will pass over the narrow mud-swamp that separates it from the river Luindi, flowing westwards to the Kamolondo River (it is not a lake), and thence to the Lualaba. Thus, what Cameron discovered, is not the present, but the future outlet of the Tanganyika, which hitherto Mr. Stanley maintains, has had no outlet. It must not be forgotten that this outlet has already been suggested by Livingstone; writing on October 8, 1871, about three years before Cameron's visit, he says, "It may be that the Loñgumba is the outlet of Tanganyika; it becomes the Luasse further down, and then the Luanio before it joins the Lualaba; the country slopes that way, but I was too ill to examine its source." The interest of geologists will certainly be excited, if not satisfied, by some references in Mr. Stanley's letter. He speaks of basalt and trap-rocks as occurring in the district, and of a large portion of the shores of the lake being composed of calcareous tufa; he also, somewhat more obscurely, refers to what he thinks may be volcanic cones; and states that considerable quantities of asphalte have been found floating on the waters of the lake. The theory which Mr. Stanley suggests as explaining the origin of this vast lake is of a sufficiently startling character. At no remote period, his hypothesis is, this part of Africa was a level table-land, westwards across which flowed the Malagarazi, and other rivers along a channel which is now occupied by the Lukuga and Luindi. But a great volcanic convulsion disturbed the region, sinking a deep hollow across the channel of the Malagarazi, which, with other streams, has ever since been filling up the bed. Mr. Stanley supposes he has come upon the lake when it has almost reached its highest level and is about to form an outlet by the Lukuga. Until more detailed and exact information reaches us concerning the structure of the country it would be premature to enter upon a discussion of this theory. Possibly Mr. Stanley's other letter, which will be published to-day, may enable geologists more fully to understand the conclusions at which the traveller appears to have arrived. In the north-west part of the lake, what Burton, Speke, and Stanley himself had thought to be an island, Ubwari, is really a peninsula. This is so indicated on Livingstone's map, with the remark that "a sandy spit connects Mozima Island and the shore."

ON Monday night Sir George Nares read a paper on some of the results and observations of the late Arctic Expedition. He discussed mainly the state of the ice and the limits of life in the most northern channels, and concluded by stating his conviction that unless the coast of Greenland extended beyond lat. 83° 20′, the Pole would not be accessible by the Smith Sound route. In the discussion which followed it was evident that a marked change of opinion has followed the results of the late expedition as to the best route to the Pole, the general opinion being that the Smith Sound route must be abandoned and that by Spitzbergen tried. This must no doubt be gratifying to Dr. Petermann, who has so long advocated the latter route, though this change of opinion is not at all inconsistent with the idea that much valuable scientific information is still to be obtained in the Smith Sound region. Admiral Richards expressed his decided conviction that sledge travelling is at an end, although he does not venture on the opinion that ships would actually *reach* the Pole by the Spitzbergen route. Now that the Presidential excitement is over in America, we may hope to hear of preparations being made for the establishment of the proposed Polar Exploring Colony.

THE National French Committee for the Exploration and Civilisation of Southern Africa has held its inaugural meeting. M. De Lesseps was appointed president. Two delegates were appointed to represent the French Section in the large Committee presided over by the King of the Belgians. They are MM. D'Abbadie, Member of the Institute, and Grandidier, the French explorer of Madagascar.

A SECOND Italian expedition for the exploration of Africa, has arrived at Suez.

THE *Honolulu Gazette* of February 28, reports an extraordinary volcanic outbreak in Kealakeakana Bay, near the entrance to the harbour. The eruption occurred at 3 A.M. on the 24th, appearing like numerous red, green, and blue lights. In the afternoon the water was in a state of peculiar activity, boiling and broken, and heaving up blocks of red-hot lava. A severe shock of an earthquake was felt by the residents of Kannakakiel during the night of the eruption.

MR. STANFORD has just published five more physical wall-maps of the series noticed by us some time since. One chief feature of these maps is their exhibition of the orography of the respective countries, and we still think it would be an improvement if the green could be dispensed with, as by gas-light it and the blue of the water are indistinguishable. We are glad to know, however, that means are being taken to remedy this in future issues, which we think may easily be done by the use of various shades of brown; when this is done the maps will leave little to be desired. They are those of North and South America, Africa, Scotland, and Ireland. Each map is up to the latest date, and as physical maps, showing at the same time all the main natural and artificial features of the various countries, they must be quite a luxury to teachers and students, and to all who have to consult maps. Africa especially is thoroughly satisfactory, and the compiler has wisely abstained from following any theory as to the course of the fragmental rivers west of Tanganyika, unless indeed one may be led to infer that he believes Lake Chad to be the receptacle of much of the drainage attributed to the Congo and the Ogové; if so, his theory, judging from the little that is known of the rivers themselves, and the various elevations of the region so clearly shown in the map, is quite as probable as any other.

M. LEVERRIER recently received a requisition from the Chamber of Commerce of Marseilles for the establishment of a special service for the Mediterranean coasts. M. Leverrier replied that the military government had established in Algeria a special meteorological service which pertinaciously refused to comply with the rules of the international service. Consequently it was impossible for him to take advantage of Algerian observations so long as the special rules of the Algerian Meteorological Service were not altered, and without Algeria no reliable service could be established at Marseilles.

THE opening meeting for 1877 of the West Riding Consolidated Naturalists' Society will be held at Pontefract on Easter Monday, April 2.

WE have to record the death, on the 9th inst., of Dr. John Scott Bowerbank, F.R.S., so well known from his important investigations on sponges. Dr. Bowerbank, born in 1797, in London, commenced life as a distiller, but being attracted by biological studies, subsequently devoted himself to them. In 1833 his first paper appeared in the *Entomological Magazine*, on the circulation of the blood in insects, from which time numerous papers by him have appeared in the *Philosophical Magazine*, the *Microscopical Journal*, the *Annals of Natural History*, the *Philosophical Transactions* and the *Proceedings* of the Zoological Society, upon the geological and structural relations of the Spongidæ, upon Pterodactyles, upon the structure of the shells of the Mollusca, and other less important points. Dr. Bowerbank was a Fellow of many learned societies, with the foundation of more than one of which he was intimately associated, including the Microscopical, the Ray, and the Palæontological.

THE tomb of Crocé-Spinelli and Sivel, the two *Zenith* aëronauts, will be inaugurated at Père la Chaise on April 4.

THE Annual Congress of the *Sociétés Savantes* will, as usual, take place at Paris on April 3 and following days.

A WELL-ATTENDED *soirée* was given by the Northampton Naturalists' Society on Tuesday evening, March 6, in the Science and Art Rooms of the Grammar School. A large and valuable series of natural objects were displayed, many of which had been collected during the past year. Amongst those especially worthy of note were a splendid collection of eggs, exhibited by the Rev. G. Nicholson; of butterflies, A. Perry, Esq.; of plants, by G. C. Druce, Esq.; and of fossils, by W. Hull, Esq., and C. Jecks, Esq. The opening address was given by the Rev. R. Winterbotham, and short addresses were delivered in the course of the evening, by the Very Rev. Canon Scott, the Rev. W. F. Aveling, and B. Thompson, Esq. A beautiful collection of photographs was exhibited by R. G. Scriven, Esq., and H. Manfield, Esq., under the superintendence of the Rev. S. J. W. Sanders, and the attractions of the evening were still further enhanced by the exhibition of several excellent microscopes, and other scientific instruments, kindly lent by various gentlemen of the town and neighbourhood.

IN "The Stone Age in New Jersey," by Dr. C. C. Abbott (Washington, Government Printing Office), a valuable mass of facts on the various implements and weapons found in that state is collected and discussed, and illustrated by upwards of 220 cuts.

PART 4 of vol. i. of the *Proceedings* of the West London Scientific Association contains, among other interesting papers, one on Waves, by Prof. F. Guthrie, F.R.S.

AMONG other matters that came before the German Ornithological Society at a recent meeting, mention was made by M. Mützel of an osprey which had lived eighty years in captivity on a farm near Neu Damm. M. Schalow took up the question whether in bad weather birds stop their migratory flight and return, or not. He affirmed that they either went on or remained for a time where they happened to be caught by the weather. M. von Schleinitz, of the *Gazelle*, stated that several individuals of *Chionis minor* had been brought from Kerguelen's Land to St. Paul's. It remains to be seen on later visits to St. Paul's whether the birds will still be found there.

M. DECHARME has been studying the comparative pitch of sounds given by various metals and alloys. Cylindrical bars of each metal were used, all of the same length and diameter (20 cm. and 1 cm.). The metals examined rank as follows in ascending series:—Lead (fa_3, 690 vibrations), gold, antimony and silver (the same), tin, brass, zinc, copper, cast-iron, iron, steel, aluminium (fa_5, 2,762 vibrations). From lead to aluminium there is thus an interval of two octaves. No simple relations, sufficiently exact, were perceived between the pitch of the sounds and the physical or chemical properties of the substances. M. Decharme's results differ considerably from those of Wertheim.

THE circumference (not the diameter) of the exploring balloons for meteorological purposes in Paris, referred to last week, is ninety centimetres. They have an ascensional force of about thirty grammes.

PROF. GARROD illustrated his Royal Institution lectures by a colossal model of a disarticulated human *skull*, not *skeleton*, as we stated last week.

THE additions to the Zoological Society's Gardens during the past week include a Puma (*Felis concolor*) from South America, presented by Commander Stanhope Grove, R.N.; a Nisnas Monkey (*Cercopithecus pyrrhonotus*) from Nubia, presented by Mr. B. C. Simpson; a Burrhel Wild Sheep (*Ovis burrhel*) from the Himalayas, deposited; two Hairy Tree Porcupines (*Sphingurus villosus*), two White-fronted Guans (*Penelope jacucaca*) from Brasil, two Blue-bearded Jays (*Cyanocorax cyanopogon*) from Para, two Turkey Vultures (*Cathartes aura*) from America, two Upland Geese (*Bernicla magellanica*) from Patagonia, three West Indian Rails (*Aramides cayennensis*), a Common Boa (*Boa constrictor*) from South America, two Great Cyclodus Lizards (*Cyclodus gigas*), a Stump-tailed Lizard (*Trachydosaurus ungosus*) from Australia, purchased; a Zebu (*Bos indicus*) born in the Gardens.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 18.—"Residual Charge of the Leyden Jar.—II. Dielectric Properties of Various Glasses," by J. Hopkinson. Communicated by Prof. Sir William Thomson, F.R.S.

The experiments appear to verify the fundamental hypothesis, viz., that the effects on a dielectric of past and present electromotive forces are superposable. Ohm's law asserts the principle of superposition in bodies in which conduction is not complicated by residual charge. Conduction and residual charge may be treated as parts of the same phenomenon, an after effect as regards electric displacement, of electromotive force. The experiments appear to show that the principle of Ohm's law is true of the whole phenomenon of conduction through glass.

February 1.—"The Meteorology of the Bombay Presidency," by Charles Chambers, F.R.S., Superintendent of the Colaba Observatory.

This work consists of four parts—the first dealing with registrations of meteorological phenomena at the Colaba Observatory during a period of twenty-seven years; the second with moderately full observations at five military stations in the Bombay Presidency during a period of nineteen years; and the third with large numbers of observations from civil hospitals and revenue stations, being those of selected registers extending over various periods from not less than a fortnight up to a number of years; in this part the phenomena treated are temperature of the air, winds, and rainfall only; and the extent of territory to which the observations refer includes the whole of the Presidency, Sind, and the western half of Rajputana. In the fourth part are discussed the general distribution (as regards both space and season) of temperature and rainfall, and the variations of the wind; first with respect simply to the physical geography of the country, and then in combination with certain theoretical views, the elucidation of which, by means of the dynamical theory of heat and the kinetic theory of gases, occupies much space.

February 15.—"On Stratified Discharges.—III. On a Rapid Contact-Breaker, and the Phenomena of the Flow." By William Spottiswoode, M.A., F.R.S.

In a paper published in the *Proceedings* of the Royal Society, vol. xxiii. p. 455, I have described a form of contact-breaker designed for great rapidity and steadiness of action. It consisted of a steel rod which vibrated under the action of an electromagnet. As regards sharpness of break and steadiness of the striæ, this instrument left little to be desired. But, as explained in the paper, an alteration in the current not only affected the steadiness directly, but also reacted on the break itself. In order to obviate this inconvenience, I devised another form of contact-breaker consisting of a wheel platinised at the edge, on which a platinum spring rests. In the circumference of the wheel a number (40 in the first instance) of slots were cut, and filled with ebonite plugs so as to interrupt the current. The breadth of the slots was about ·04 inch, and that of the teeth about ·5 inch. The wheel was connected with suitable driving gear, so as to give from 250 to 2,000 currents from the coil in each direction per second.

For some time the experiments were conducted with the platinum spring resting on the wheel; and the effects were varied by altering either the pressure of the spring or the velocity of the wheel; but it was found that actual metallic contact between the spring and wheel was not necessary, provided that a layer or cushion of conducting material were interposed. Such a layer was formed by a thin film of dilute sulphuric acid drawn out by a thread leading from a reservoir and resting on the wheel. The spring, which under these circumstances became unnecessary, was replaced by a point, the adjustment of whose distance from the wheel was simple. This arrangement gave excellent results, even when the number of currents per second was reduced in some cases to 250; added to which the unpleasant and disturbing noise of the friction was entirely avoided. Ultimately, however, I used a light wire in the place of the spring first mentioned.

With the contact-breaker here described effects similar to those produced by the rapidly-vibrating break were obtained. The striæ were formed in a regular manner, and advanced or receded, or remained at rest, in a column usually unbroken so long as the velocity of the wheel was maintained without change.

With a view of ascertaining the nature of the distinction between the ordinary and the steady striæ, careful observations were made with the revolving mirror. It having been noticed that when the wheel-break moved slowly, ordinary or irregular striæ were produced, and that when it moved rapidly, steady striæ resulted, it seemed probable that the latter effect might be due to the short time of contact, and to the consequent absence of many of the features described in Part II. of these researches. This is, in fact, identical with the suggestion there made, that the fluttering appearance was due to the unequal duration of the striæ themselves, and to the irregular positions of the points at which they are renewed at successive discharges of the coil. And such, in fact, proved to be the case; for as the speed of the wheel was increased, the duration of the discharges diminished; the image as seen in the mirror became narrower and simpler in its configuration, until, when the steady effect was produced, each discharge showed only a single column of striæ of a width proportional to the apparent width of the slit. The proper motion, implied by the inclination of the individual striæ to the vertical, was still perceptible, and was directed as usual towards the negative pole.

The phenomena of the flow may be considered to be due to the different positions taken up by the striæ in successive discharges. If in each discharge the striæ occupy positions in advance of those occupied in a previous discharge, the column will appear to advance; if the reverse be the case, they will appear to recede. If the positions remain unchanged, the column will appear stationary.

Experiments were instituted with a view of ascertaining the connection between the flow and resistance. Starting from a condition of current and break for which the striæ were stationary, it was found that an increase of resistance, introduced generally in the primary circuit, produced a forward flow, *i.e.* from the positive towards the negative terminal, while under similar circumstances a decrease of resistance produced a backward flow. Furthermore, if after producing a forward flow the resistance be continually increased, the flow, after increasing in rapidity so as to become undistinguishable by the unassisted eye, gradually appears to become slower, and ultimately to reverse itself.

Another form of contact-breaker was also occasionally used. The principle upon which it was based was the sudden disruption of a thin film of conducting liquid by a discharge between the electrodes of a circuit.

As soon as the current passes, the fluid between the plate and point will be decomposed, and electrical continuity broken. This done, the fluid flows back again, and continuity is restored.

February 22.—"On a New Form of Tangential Equation," by John Casey, LL.D., F.R.S., Professor of Higher Mathematics in the Catholic University of Ireland.

"Addition on the Bicircular Quartic," by A. Cayley, LL.D., F.R.S., Sadlerian Professor of Mathematics in the University of Cambridge.

Geological Society, March 7.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—The Rev. Ebenezer Davies, William Davies, and Henry Davis Hoskold were elected Fellows, and George Garves Brush, Professor of Mineralogy in the Sheffield School of Science, Yale College, Newhaven, Connecticut, Prof. A. L. O. Descloizeaux, of Paris, Prof. E. Renevier, of Lausanne, and Count Gaston de Saporta, of Aix en Provence, Foreign Correspondents of the Society.—The following communications were read:—On the vertebral column and pelvic bones of *Pliosaurus evansi* (Seeley), from the Oxford Clay of St. Neot's, in the Woodwardian Museum of the University of Cambridge, by Harry Govier Seeley, F.L.S., Professor of Geography in King's College, London. In this paper the author described some bones obtained by J. J. Evans in the lower part of the Oxford Clay at Eynsbury, near St. Neot's. They consisted of thirty-seven vertebræ, twenty-one of which are cervical, and apparently complete that series. These presented the characters of the cervical vertebræ of the typical Pliosaurs of the Kimmeridge Clay. The remains of the pelvis included a pubic bone showing a close correspondence in form with those of the Pliosaurs of the Kimmeridge Clay of Ely, and an ischium.—Supplementary notes on the fauna of the Cambridge Greensand, by A. J. Jukes-Browne, F.G.S. This paper was supplementary to one communicated to the Society by the author in 1875, in which he maintained that the Upper Greensand does not extend further in a northwesterly direction than West End Hill, near Cheddington, in Buckinghamshire, that the Cambridge Greensand is merely a nodule-bed at the base of the Chalk Marl, resting unconformably upon denuded Gault, to the upper part of which the greater portion of the fauna belongs, and that the

remainder of the fauna, belonging to the deposit itself, consists of species proper to the Chalk Marl rather than to the Upper Greensand. The object of the paper was to indicate certain additions to, and corrections in, the list of fossils upon which these conclusions were supported. The following Gault species were indicated as not previously identified in the Cambridge Greensand :—*Nautilus arcuatus*, Desh.; *N. inæqualis*, Sow.; *Turrilites elegans*, D'Orb. ; (?) *T. emericianus*, D'Orb. ; *Ornithopus histochcila*, Gardn. ; *Brachystoma angularis*, Seeley ; *Turbo pictetianus*, D'Orb. ; *Pleurotomaria regina*, Pict. and Roux ; (?) *P. itieriana*, Pict. and Roux ; *Pecten raulinianus*, D'Orb. ; *P. subacutus*, D'Orb. ; and *Lima rauliniana*, D'Orb. The author described as new species :—*Turrilites nobilis*, *Nautilus*, sp. nov., *Natica levistriata*, *Nerita nodulosa*, and *Lima interlineata*, and noted several corrections in the nomenclature adopted in his former list.—On the beds between the Gault and Upper Chalk, near Folkestone, by F. G. Hilton Price, F.G.S. The author described the characters presented by the beds between the Gault and Upper Chalk near Folkestone, indicated the fossils contained in them and their range in this division of the Cretaceous series, and discussed the classification of the deposits and their equivalence with those recognised by other writers.

Anthropological Institute, March 13.—Mr. John Evans, F.R.S., president, in the chair. The President exhibited a hafted bronze celt, with its original wooden handle (which was covered with brass plates), found near Chiusœ.—Mr. Biddulph Martin exhibited some pottery, shells, and other remains from a supposed kitchen midden at Smyrna, which, the president pointed out, was of comparatively modern date. Some flint arrow-heads, scrapers, &c., from Ditchley, were exhibited by Capt. Dillon.—Mr. Hyde Clarke then read a paper on the Himalayan origin of the Magyar. The object of this paper was to show that languages of Nepal, &c., in the Himalayas, formerly called Subdravidian, are to be classified as Ugrian, and include Finnish, Magyar, Lap, and Samoyed affinities. In connection with the extension of the Ugrian area and possible centre to High Asia, the author entered on the question of the origin of the Magyars. After referring to the Magyar, Khun, &c., in the Himalayas, he proposed as a solution that the attack on Pannonda had been made by Avar or Khunzag traders from the Caucasus speaking a Vasco-Kolarian language, and with a main body of Ugrians, the language of whom prevailed on the extinction of the former. The author dissented from the Ugrian classification of Accad and Etruscan, giving other prehistoric examples for the Accad words in Lenormant, claimed by him and M. Sayous as Ugrian. Messrs. Bertin, Salymos, Rees, the President, and others, took part in the discussion.—The Director then read the following papers by Mr. Hector McLean :—On the Scottish Highland language and people, and on the Anglicising and Gaelicising of surnames.

Physical Society, March 3.—Prof. G. C. Foster, president, in the chair.—The following were elected members of the Society: —Mr. J. A. Fleming, Mr. P. le Neve Foster, and Mr. S. Hall. —Prof. Foster showed experimentally the polarisation of heat rays, employing two large Nicol's prisms of $2\frac{5}{8}$ inch aperture, and a thermopile surrounded by a double jacket and connected with a Thomson galvanometer as arranged by Mr. Latimer Clark for showing very slight indications to an audience. When the principal sections of the prisms were at 90° to each other only a slight movement, doubtless due to an initial heating of one side of the pile, was observed ; and the amount of the deflection was found to increase steadily up to about sixty divisions on the scale as the above angle was diminished. Prof. Foster exhibited the results of experiments made to determine the intensity of a source of heat by this means, and they were very concordant.—Mr. Latimer Clark then explained the arrangement of the galvanometer used. The image of an arrow-head or other form of index projected by means of a limelight at the further end of the room traverses a telescopic object-glass about two feet distant from the lamp and falls on a square silvered plate of glass suspended from the needle of a Thomson galvanometer, which is rendered steady in the ordinary way by a platinum spade in water. The reflected image then traverses the whole length of the room and falls on a large scale placed in front of the audience, and, by such an arrangement, the instrument may be at any distance from the scale, and yet the image will not be unduly magnified. A method is employed for bringing the needle rapidly to rest. A few thermo-electric couples are placed above the lamp chimney, thus being kept constantly hot, and the terminals are united by a wire which is coiled several times round the galvanometer; the circuit is closed at the moment when this subsidiary current will tend to neutralise the motion of the needle.—Prof. Guthrie incidentally mentioned that the difficulty experienced in separating the fibres of a cocoon thread may be obviated by boiling the thread in carbonate of potash, when the natural resin is saponified and the fibres may be easily split.—Mr. Wilson then explained some difficulties he has met with in constructing a Holtz electrical machine, especially with reference to the windows and armatures, and he exhibited two machines which he recently made, from one of which a spark five or six inches in length can be obtained ; this apparatus is so arranged that it can be taken entirely to pieces and packed in a very moderate-sized case. After carefully pointing out the difference between an ordinary machine and the Birch machine, he proceeded to consider the theory of the Holtz machine, and explained how he was led to construct an instrument in which there were no windows, the armatures being placed on the face of the fixed plate next to the moving plate, but the result was not satisfactory. He then made the larger machine provided with six fixed and six moving plates, and the windows were replaced by holes $\frac{1}{8}$ inch in diameter traversed by short pieces of tape glued to the paper armatures. The initial charging of the armatures is effected by means of a disc of ebonite fixed to the main axis of the machine, which is lightly held by the fingers and caused to rotate. Electricity is thus generated and points projecting towards it and communicating with points in the neighbourhood of the armatures cause them to become charged ; after this, electricity is generated with great rapidity.—Prof. MacLeod gave some details concerning the working of a large Holtz machine which he drives by a turbine. He finds that after it has been in action for nearly an hour a much greater force is required to work it, and he suggested a theory in explanation of this phenomenon. By keeping the machine dry under a glass shade reversing effects are entirely avoided as well as the necessity for varnishing the plates.

Entomological Society, March 7.—J. W. Dunning, F.L.S., vice-president, in the chair.—Mr. Douglas exhibited a specimen of the Longicorn Beetle, *Monohammus sutor*, brought to him alive, having been captured in a garden in the Camden Road ; also a melanic variety of *Orthosia suspecta*, taken at Dunkeld.—Mr. Hudd exhibited some interesting varieties of British Lepidoptera taken near Bristol and in South Wales ; amongst them were *Lycæna alexis*, *Sphinx ligustri*, and *Boarmia repandata*, the latter a black variety.—Mr. Champion exhibited specimens of *Cardiophorus rufipes*, a species new to Britain, taken by Mr. J. Dunsmore, near Paisley ; also a British example of *Aphodius scrofa* from the collection of Mr. Dunsmore, who unfortunately had no note of its locality.—The Secretary exhibited a specimen of an Isopod Crustacean which had been forwarded to him by Mr. J. M. Wills, surgeon, s.s. *City of Canterbury*, who stated that it was found occasionally parasitic on the flying-fish, and generally close to the pectoral fins. Mr. Douglas read an extract from a letter from Dr. Sahlberg, who had recently returned from an excursion to the neighbourhood of the Yenisei River and the extreme north of Siberia, from whence he had brought a large number of insects, principally Coleoptera and Hemiptera. Amongst the Hemiptera were one *Aradus*, one *Calocoris*, two *Orthotylus*, one *Orthops*, one *Pachytoma*, one *Anthocoris*, one *Acompocoris*, five *Salda*, and one *Corixa*, which appeared to be hitherto unknown. The species of *Salda* were from the extreme north, in Tundra territory.—The Secretary read a paper by Mr. W. L. Distant, on the geographical distribution of *Danais archippus*, a North American butterfly which has recently been taken in the south of England.

Mineralogical Society, March 14.—Mr. H. C. Sorby, F.R.S., president, in the chair.—The following papers were read :—On a simple method for roughly determining the index of refraction of small portions of transparent minerals, by H. C. Sorby, F.R.S.—This can be effected by having a small graduated scale attached to the body of the microscope, by means of which the thickness of the crystal and the displacement of the focal point can be easily measured. From these data the index of refraction can be at once calculated with sufficient accuracy to make the result valuable in determinative mineralogy.—On a serpentine from Japan, by A. H. Church, M.A.—Notes on Vauquelinite from Scotland, and Cantonite or Harrisite from Cornwall, by Thomas Davies, F.G.S.—On an easily constructed form of reflecting goniometer, by S. B. Hannay, F.C.S.—On a peculiar form of quartz crystals from Australia, by Rev. J. M. Mello, F.G.S.—On certain black quartz crystals from Boscaswell Down, Cornwall, by

J. H. Collins, F.G.S. The black colour is due to minute crystals of schorl.—On quartz including oxides of iron, by William Vivian.—On the magnetic constituents of minerals and rocks, by J. B. Hannay.—On the water contained in minerals, by J. B. Hannay.—On the Nordenskjöld iron masses from Greenland, by K. T. V. Steenstrup, translated by Mr. Rohde, one of the Danish expedition. The author contends that the iron is a natural constituent of the basalt, and not of meteoric origin.

Institution of Civil Engineers, March 13.—Mr. George Robert Stephenson, president, in the chair.—His Majesty the King of the Belgians was elected by acclamation an honorary member. The paper read was on the transmission of motive power to distant points, by Mr. H. Robinson, M. Inst. C.E.

MANCHESTER

Literary and Philosophical Society, December 4, 1876.—Charles Bailey in the chair.—Notes on a botanical excursion in the Aberdeenshire Islands in July, 1876, by Mr. Thomas Rogers.

December 26, 1876.—A notice of some organic remains from the schists of the Isle of Man, by E. W. Binney, president, F.R.S., &c.

January 9.—E. W. Binney, F.R.S., F.G.S., president, in the chair.—On the poisonous properties of yew-leaves, by James Bottomley, D.Sc.—On the luminous sulphides of M. Ed. Becquerel, by William Thomson, F.R.S.E.—On the types of compound statement involving four classes, by Prof. W. K. Clifford, M.A., F.R.S. Communicated by Prof. W. S. Jevons, M.A., F.R.S.

January 23.—Results of the monthly observations of the magnetic dip, horizontal force, and declination made at the magnetic observatory of the Owens College, from January, 1874, to December, 1876, inclusive, by Prof. Thomas H. Core, M.A. Communicated by Prof. Balfour Stewart, LL.D., F.R.S.

GENEVA

Physical and Natural History Society, February 1.—M. Alphonse Favre presented a geological map of the Canton of Geneva on the scale of $\frac{1}{25000}$, intended to enlighten agriculturists on the management of the soil for their various crops.—Prof. Schiff described the researches he had made on the properties of nicotine as a poison, and on the part played by the liver in such poisoning.

February 15.—M. E. Renevier, professor at Lausanne, exhibited to the Society his geological map of Vaudoises Alps, on the scale of $\frac{1}{50000}$, as also several sections which complete it. It includes principally the mass of the Diablerets and the neighbouring spurs on the right bank of the Rhone.—Prof. Wartmann showed a small apparatus intended to prove the impulse which an induction spark in a rarefied gas is capable of giving in the direction of its length.—M. Raoul Pictet described various experiments made by himself, and proving the great facility with which sulphuric acid is diffused through caoutchouc.

PARIS

Academy of Sciences, March 19.—M. Peligot in the chair.—The following papers were read:—Observations of temperature at the Museum of Natural History, during 1876, with electric thermometers placed at depths of 1 metre to 36 metres under ground, as also in air and under grass-covered and bare ground, by MM. Becquerel. The results are nearly the same as were obtained before. General Morin, remarking that at only 10 to 12 metres depth he found a nearly constant temperature of 11°, suggested the use of underground air, drawn through pipes, to produce a constant temperature where it might be required (as for conservation of meat, &c.)—On the decomposition of bioxide of barium *in vacuo*, at the temperature of dark red, by M. Boussingault. The whole of the oxygen could be thus extracted. On removing the source of heat the bioxide was reconstituted. Bioxide of barium, then, cannot exist *in vacuo* at a dark red heat.—Physical and mechanical actions of incandescent and strongly compressed gases from combustion of powder. Application of the facts to certain characters of meteorites and bolides, by M. Daubrée. The facts of experiment seem to explain:—(1) the alveolar cavities or cupules in meteorites; (2) the peculiar clouds, smoke, or dust, which follow the disappearance of the incandescent mass; (3) the dust of cosmic origin which is thus expanded in our atmosphere, not only by combustion and volatilisation, but by pulverisation at a high temperature.—On the fundamental invariants of the binary form of the eighth degree, by Mr. Sylvester.—On the palæontological origin of trees, shrubs, and bushes indigenous in the south of France, that are sensitive to cold in severe winters, by M. Martins. He considers them to be the survivors of the flora which covered those parts during the Tertiary period. We have here plants exotic as regards *time*, while other plants are exotic as regards *space*. At Montpellier and Marseilles the annual average of absolute minima of temperature are − 9·23° and − 5·95° respectively.—On the experiments made at Prégny by the canton of Geneva, by M. Bouley. He had erroneously given the Swiss Federal Government the credit of the experiments.—M. Hebert was elected member in the section of Mineralogy in room of the late M. Ch. Sainte-Claire Deville.—On the phosphorescence of organic bodies, by Mr. Phipson. *Apropos* of a recent note, he calls attention to his memoir on noctilucine, the first organic body known to be phosphorescent by slow oxidation, like phosphorus in the mineral kingdom.—Propositions of algebra and geometry deduced from consideration of the cubic roots of unity, by M. Appell.—On the curvature of surfaces, by M. Serret.—On a problem comprising the theory of elimination, by M. Ventéjols.—On the suspension of water in a vessel closed below by a tissue with large meshes, by M. Plateau.—On a singular fact of production of heat, by M. Olivier.—On the reform of some processes of analysis used in the laboratories of agricultural stations and observatories of chemical meteorology; first part, Ammonimetry, by M. Houzeau. His method is to use stable vinous red litmus, which will reveal free ammonia in solution in water containing only $\frac{1}{200000}$ and even $\frac{1}{400000}$ of its weight of that substance. The proportion is determined by pouring into the litmus-coloured liquor a weak titrated acid till disappearance of the original red.—On the preparation of crystallised acetate of magnesia, and on the fermentation of this salt, by M. Patrouillard.—On a simple mode of production of certain mono-, bi-, and tri-chlorised acids, by M. Demarçay.—Transformation of normal pyro-tartaric acid into dibromo-pyrotartaric and dibromo-succinic acids, by MM. Reboul and Bourgoin.—Action of chlorochromic acid on anthracene, by M. Haller.—On the constitution of pseudo-purpurine; continuation of researches on the colouring matters of madder, by M. Rosenstiehl. Pseudo-purpurine is sufficiently unstable to produce by its partial destruction the other colouring matters in madder (alizarine excepted).—Experiments on muscular tonicity, by M. Carlet. A muscle generally contracts after section of a nerve, before final relaxation. This is due, the author says, to the increased excitability of the nerve. In rare cases, where the nerve is cut without excitation (at one of Bridge's "nodes" perhaps), relaxation begins at once. Elongation immediately after section is due to *rigidity* not *flaccidity*, the extensor muscles being stronger than the flexors.—On the modifications in the egg of phanerocarpous medusæ before fecundation, by M. Giard.—On the age of elevation of the Margeride, by M. Fabre.—On the formation of thunderstorms, by M. Zundel.—Clinical and therapeutical researches on epilepsy and hysteria, by M. Bourneville. From two analyses of portions of liver from an epileptic patient who had taken, in four months, forty-three grammes of ammoniacal sulphate of copper (and died of pulmonary tuberculosis 2½ months after cessation of the treatment), the total amount of copper in the liver was estimated at 236 milligrammes and 250 mm.

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